

THE PSYCHOLOGICAL REVIEW.

SOME PECULIARITIES OF THE SECONDARY PERSONALITY.

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Of the many unsolved problems in psychology, that of automatism is perhaps the most baffling. Automatic utterances, whether in the form of writing or the speech of the so-called trance-medium, present certain peculiarities which distinguish them so clearly from the utterances of normal subjects as to require some special explanation. Other abnormal mental conditions, such as mania, melancholia, hypnosis, or hallucinations, present peculiarities each of its own kind, but these are by no means so puzzling as those of automatism. If not at present fully explained, we believe that they may be eventually understood as exaggerations or perversions of normal forms of mental life. In automatism, however, we are apparently confronted with phenomena of a different kind. They belong to that class which the scientist of the day would call 'remarkable,' demanding instant attention and careful verification, and requiring if they persist some special explanation. Indeed the extremely striking character of some of the phenomena of automatism may be illustrated by the nature of the hypotheses that have been made to explain them. I have in mind, in particular, one series of automatic utterances which have been under investigation for nearly fourteen years by psychologists trained in scientific methods, and at the end of this time one of these psychologists, who has been most intimately connected with the investigation, reckoned to be a man of sanity and careful

logical habits, has proposed as the only hypothesis capable of explaining the facts, that the person from whom the utterances come is 'controlled' by one or more disembodied 'spirits' of the deceased.¹

Such a hypothesis violates almost all the conditions to which a legitimate hypothesis should conform. It does not connect the phenomena in question with any other known facts or laws. Proposing as the basis of explanation certain wholly unknown forms of being, it admits of no deductive inference of consequences. It can not, furthermore, be clearly and definitely conceived, and does not, finally, explain all the facts. I mention this merely to illustrate the straits which psychologists are in to explain the phenomena of automatism. The peculiarity of the situation is not greatly lessened when we learn that other psychologists maintain in all seriousness that, without recourse to the 'spirit' hypothesis, the phenomena may all be explained by 'telepathy'—a doctrine itself of questionable antecedents.

Under these circumstances, what should be the attitude of psychologists towards automatism? No one can doubt the answer which every scientist would make to this question. We want more facts and more hypotheses—especially facts. While this is the true attitude, unfortunately it is not the one which has usually been held. Too many have treated the whole subject with neglect if not with actual contempt. This wholly unscientific attitude has been the result of no real want of the spirit of investigation on the part of psychologists; it has been due rather to the frowns of the science world in general, and this again is explained if not excused by the unhappy history of the phenomena of automatism. These bear, at least in England and America, the corrupting marks of evil associations. They suggest all sorts of charlatanry and superstition. It has been felt that to maintain the dignity of experimental psychology, this subject and certain related ones must be ignored. They have been almost uniformly kept out of American psychological laboratories, where infinite labor has been spent upon other

¹ 'A Further Record of Observations of Certain Phenomena of Trance.' By Richard Hodgson, LL.D. Proceedings of the Society for Psychical Research, February, 1898.

probably less fruitful problems. But experimental psychology has now long passed its probationary period and may quite freely choose its subjects for research, and at present there is perhaps no other subject promising to throw more light upon certain dark chapters in mental science than that of automatism. No one can read the reviews that have appeared of Dr. Hodgson's report upon the trance-utterances above mentioned, indicating the self-confessed confusion of those most intimate with the case, coupled with a half readiness to accept almost meaningless explanations, without feeling the urgent need of a wider acquaintance with related facts. To be sure, the psychical research societies and a number of distinguished and devoted French investigators have been for some years assiduously cultivating this field. Whether we judge the results of their labors, however, by the conclusion lately arrived at by Dr. Hodgson, or by the excellent summary contained in Mr. Podmore's recent *Studies in Psychical Research*, the conviction is strengthened anew that we want more facts and more explanations. Without denying its great debt to psychical research, experimental psychology may profitably take up the problem of automatism, apply still more rigorous methods, and, what is of greater importance, include in its investigations a larger number of more simple cases. In the psychical research literature, one is wearied by the perpetual recurrence of a few remarkable 'classical' cases. It would be desirable to have fresh cases, a good many of them, and they should be simple ones. The cases reported hitherto have been too complex and remarkable, or rather the examination has not included a sufficient number of the less complex and less remarkable. The primary object of the present paper is to urge the extension of experimental work in this direction. The thorough study of simple cases of automatic writing and of all forms of automatism in normal healthy subjects is wholly practicable in the laboratory and certainly desirable. Encouraging beginnings have been made in American laboratories by Solomons and Stein¹ in two researches upon normal motor automatisms, and by Jastrow² and Tucker³ in researches upon involuntary movements.

¹ *PSYCHOLOGICAL REVIEW*, Vol. III., p. 492. Ibid., May, 1898.

² *American Journal of Psychology*, Vol. IV., p. 398.

³ *American Journal of Psychology*, Vol. VIII., p. 394.

About three years ago I undertook, as a contribution to this subject, to make a study of a simple case of automatic writing. Owing to the absence of the 'subject' from the city for two years, the study was only recently completed. I present it now rather as an indirect means of furthering my object above mentioned than as a study possessing any intrinsic value in itself. For this reason I add certain details of procedure, which, while familiar to every 'psychic researcher,' may perhaps be useful to the larger body of investigators whom I conceive to be demanded by the importance of the problem. I wish also to use the occasion to call attention to certain peculiarities of the secondary personality appearing in this and in other cases, and incidentally to notice their relation to certain hypotheses that have been made to explain them. I shall, therefore, rather freely preface the account itself with some general remarks and some mention of other experiments that I have made. I use the term 'secondary personality' advisedly, finding it preferable to secondary consciousness, or subliminal or subconscious personality, or any other phrase, as it is justified by the facts, and is in harmony with any, even the 'spirit,' hypothesis. In automatic writing, for instance, we find ourselves in communication with a source of intelligence that hears and answers questions, reasons, exhibits pleasure and anger, assumes a name which it retains from day to day and from year to year, and displays an accurate memory extending over long intervals of time. To such a source of intelligence we cannot refuse the name of personality. When in connection with the same physical organism we find a synchronous or alternating intelligence, exhibiting different mental peculiarities, having a different name and displaying a different set of memories, we find it not only convenient but suitable to speak of a primary and a secondary personality. This secondary personality may be an apperceptive unity corresponding to a special grouping of association tracts in the subject's brain, it may be some lower mental stratum belonging to a sort of universalized psychic faculty, or it may be the 'spirit' of my deceased grandfather; it may or it may not be subliminal; it is even conceivable that it should not be conscious, but it bears all the common marks of personality.

Thus far the problem presents no very serious difficulty. The mere fact that there should be in connection with the same organism two personalities is not more wonderful than that there should be one. There is nothing in our present knowledge of the ego either from the psychological or physiological standpoint preventing us from admitting that the elements which usually join in a single group may, under certain conditions, so associate themselves as to form two or three or any number of different groups, nor, indeed, that the same elements, as, for instance, memory images, may at once form a part of both or of several systems. Furthermore, there is another circumstance which would seem to make the scientific study of the secondary personality at least possible. It has certain pretty clearly defined marks, traits, or peculiarities capable of logical description. The presence of these traits in all the cases of automatism which have been reported forces upon us the conviction that they all belong to the same general class and that the investigation of the simpler cases may throw much light upon the more complex ones. If we compare a simple case of automatic writing, such as may be found in one of almost any company of schoolgirls, with the wonderful case reported by Dr. Hodgson, the difference is as great as between a kitten and a tiger, but perhaps not greater, for a careful observer will discover 'marks' which indisputably place them in the same genus. What we need now is a more complete description of these marks. Besides the case presented below, I have recently had opportunity of studying two other cases of automatism, both instructive, neither of them very remarkable, and in all of them I have been impressed by the presence of the usual marks, for instance, suggestibility, fluency, absence of reasoning power, exalted or heightened memory, exalted power of constructive imagination, a tendency to vulgarity or mild profanity, the profession of 'spirit' identity and of supernatural knowledge, and, finally, a certain faculty of lucky or supernormal perception difficult to name without committing oneself to a theory, which, therefore, we may call a kind of brilliant intuition. It seems to me not impossible ultimately to make a complete list of these marks, and then, perhaps, to explain why they are character-

istic of the secondary personality. Some time ago I paid a visit to a 'medium' residing in a small western city. She is a married woman with a family, and was made known to me by one of my students whose family was intimately acquainted with the woman, having known her from her girlhood. My investigation left no doubt in my mind that she is an honest woman and passes into a genuine trance, and upon awakening is ignorant of her trance-utterances. These take the form of the personality of a Quaker doctor or of a little girl named Emma, both professing themselves to be 'spirits' of deceased persons, and to have supernormal and supernatural knowledge. I conversed for an hour with 'Emma,' and was throughout struck by the remarkable likeness in the general form of the utterances to those more remarkable ones recorded by Dr. Hodgson and others, so that I cannot doubt that we have to do with phenomena of the same genus and species, and that the explanation of the simpler case, were it at hand, would throw much light upon the more complex one. The similarity extended even to an accurate and astonishing statement made (as so often happens) at the very beginning of the sitting about my place of residence and my occupation. This was certainly an interesting trait and in need of explanation, although it would not have suggested to me the hypothesis that 'Emma' was the 'spirit' of a real person, for, however difficult it might be for a woman who had apparently never seen or heard of me before to tell me my home and occupation, it would evidently be more difficult for a young girl to do so who had lived and died prior to the circumstances and relations mentioned. If we have to ascribe to our communicator powers of perception transcending time and space, it makes our hypothesis needlessly complex to ascribe them first to a 'spirit' and then locate the 'spirit' in the subject before us. If we ascribe them directly to our subject we avoid the trifling inconvenience of supposing that things are known before they happen, or, if we must violate time and space, we have to violate less of both.

Again, not long ago, I became acquainted with a young girl who was an automatic writer and whom I had several opportunities of studying. She wrote rapidly and legibly, only requir-

ing that some other girl should hold the pencil with her. I convinced myself that the writing was purely automatic. It usually purported to come, and was sincerely believed by the girl to come, from the 'spirit' of her deceased mother. I shall mention one or two characteristic utterances from this case, but what I wish to emphasize is merely that the general form of the utterances was so similar to the others which I have studied and to those referred to above, that I cannot doubt that we have here again to do with closely related if not identical phenomena, and that the full explanation of the one would remove the mystery from the other. In all the writing which I saw from this subject (I shall mention some other examples from it below), there was one utterance and one only of the brilliantly intuitive type, and this again came early in the first sitting. In response to my questions, the correct answer was received that I had three sisters and two brothers, that the brothers were both younger and one of the sisters younger and two older. In response to my inquiry about their names, one of my sisters' names, a common one, was given, and then 'Gussie' was written which was spontaneously changed to 'Bessie,' the latter being correct. Admitting that the chances of correctly guessing such a combination as the above at the first guess are too small to make that a probable explanation, and admitting that the young girl, who was an entire stranger to me at the time, could not have known in any normal way what the most intimate friend that I had in the city could hardly have known, what is the most that can be made out of such an utterance? If found to be a real intuitive utterance, not conforming to the usual laws of perception, memory, or constructive imagination, and if found to be similar to a sufficient number of other automatic utterances, it becomes an interesting mark of the secondary personality, but so far as I can see is not consistent with one more than another of the various hypotheses that have been offered. Probably no thoughtful investigator would apply the 'spirit' hypothesis, for instance, here, but so vitiated have we become in our logical methods when we enter the field of psychical research, that it seems to be generally accepted that if we could adopt this hypothesis it would explain utterances of this class. But, however difficult it might

be to understand how the young girl could have known about my family, it would be still more difficult to believe that her deceased mother, who had never even heard of me, could have known, and there was no time to ascertain by inquiries. It is easy for the popular mind to understand all sorts of telepathic, clairvoyant, and time-obliterating powers when attributed to 'spirits' instead of every-day people, and the history of philosophy, despite the warnings of William of Occam, is full of that kind of reasoning. It has become very rare, however, in modern science. The 'spirit' hypothesis accounts for these peculiar phenomena of automatism in the same way that Descartes' 'animal spirits' accounted for the interaction of mind and body, or that the mythological tortoise explained the supporting of the world. From the logical point of view, however, it seems to me that little better can be said of Mr. Myers' theory of a 'spectrum of consciousness indefinitely extended at both ends,' with its 'telepathic and clairvoyant impressions,' 'falling under some system of laws of which supraliminal experience could give us no information' and 'transcending in some sense the limitations of time as well as of space,' having powers 'subject, not to the laws of the known molecular world, but to laws of that unknown world in which the specific powers of the subliminal self are assumed to operate.' This is a metaphysical, not a psychological hypothesis.

The subject of the experiments which I wish to mention in more detail is a young man, 22 years of age at the time the experiments began. I shall speak of him in the following account as Henry W. He is now a graduate of the University of Iowa, a young man of unquestioned integrity, a quiet and intelligent student, standing high in his class and respected by all who know him. His parents are honest farming people, both native Americans. He has never exhibited any signs of abnormality of any kind, excepting the automatism to be described. He has good physical health and mental balance. Neither he nor his parents are spiritists. He has an aunt, however, who is a spiritist, and about four years before these experiments were begun he had some conversation with her upon the subject and probably opened some books relating to it. This, however, he

says, made no impression upon him, and if he casually heard or read at that time any spiritistic phrases, such as 'pass out' for 'die,' he has no conscious recollection of them. He has no interest in the subject and has regarded it, so far as it has entered his thoughts at all, as a curious superstition. About the time of the beginning of the experiments, he became interested in hypnotism, and attended two or three times the performances of a travelling hypnotist, offered himself as a 'subject,' and proved to be an excellent one. He had never previously been hypnotized.

Shortly after this, having read of post-hypnotic suggestion, he inquired of me about it, and at his request I made a trial of it with him. Hypnosis was readily induced by a few suggestions, and I told him that exactly five minutes after he awakened he would go to the next room, secure a book from a desk and bring it to me. A few other simple tests were made which, though commonplace in themselves, should be mentioned here for reference later. Hallucinations, both positive and negative, were readily induced. I suggested that a small barbed-wire fence was stretched across the floor, over which it would be necessary for him to step carefully. This hallucinatory fence he saw and stepped over with great care. Upon awakening he remembered nothing of what he had heard or done. Exactly five minutes after awakening he carried out in detail the suggestion about the book. A few days after this, the subject of automatic writing having come to his attention, Henry W. incidentally mentioned to me that when he held a pencil idly in his hand, his hand moved continuously, making scrawls but never writing anything. I therefore made an appointment with him for the study of automatic writing. Three sittings were held and then a period of two years intervened. Then followed three more sittings. All were held on Saturday mornings. The procedure at each morning's sitting was as follows: I provided a quiet room and one assistant. At the second sitting only, others were present. A plentiful supply of very large sheets of smooth brown paper was provided. The subject was so seated with his right side toward the table, that his body was slightly turned away. His right hand held an ordinary pencil

in an easy position on the paper.¹ His head was turned slightly to the left, and he held in his left hand an interesting storybook or sometimes the morning paper, which he read and to which he was instructed to give his whole attention. No screen was used, as the subject could not see the writing without turning his head. The sittings lasted two or three hours with intervals of rest. The writing was usually quite clear, but occasionally illegible. If illegible, the communicator was asked to write the answer again. At one time I suggested to the communicator that he was a good penman, his chirography being round, clear and rapid. Instantly it became so and gave us no more trouble at the time. Henry W. never knew what he had written without reading it, except in a few instances when, his attention being allowed to wander from his book or newspaper, by following the movements of his hand he could tell something of the communication. He was much interested in the writing and was occasionally allowed to look at it. When it was nearly illegible he was never able to decipher it better than the others. The questions were either prepared beforehand and numbered or else taken down and numbered by the assistant, who also numbered the answers as written. My space will not permit me to give more than a portion of the questions and answers, nor would it be profitable to do so. They may be classed in three groups: Those of the first group were intended to bring out all the information possible about the communicator himself, his past his-

¹ I have never found the ordinary planchette of any use in automatic writing. When it is discovered that two persons succeed better in writing than one, both may grasp a common lead pencil, one hand above the other. The instrument used by Professor Jastrow, consisting of a glass plate upon glass-marble rollers, whether used for automatic writing or any involuntary movements, has the disadvantage of moving by its own momentum when once started. When it is necessary to 'educate' from the beginning an automatic writer, a delicate planchette mentioned by Miss Stein may be used. It consists merely of a board swung from the ceiling by a small wire. The one used in our laboratory consists of a light board six inches square, upon which the fingers rest as upon the common planchette. Through the board is a hole fitted with a glass tube in which a pencil is placed so that it will move up and down. A weight attached to the top of the pencil keeps it pressing lightly and evenly upon the paper below. Such a planchette swung from the ceiling over the table, will glide around upon a large sheet of paper with the slightest effort, the pencil point always leaving its tracings.

tory, his present mode of existence, his mental habits and his emotional peculiarities. The second group was intended to test his professed supernormal knowledge. The third group was directed to possible remarkable powers, such as telepathic knowledge, mathematical ability, hypermnesia and prophecy. The questions of the first group were connected more directly with the object of my inquiry. No remarkable telepathic or intuitive powers were discovered. If such powers had been found, they would have been of interest, but hardly more important for gaining a thorough knowledge of the secondary personality than more simple if less striking traits.

The first sitting opened as follows:

Q. Who are you?

A. Laton.

This was illegible, and Henry W. was allowed to look at the writing. He read it as 'Satan' and laughed. A further series of questions revealed the name as 'Laton.'

Q. What is your first name?

A. Bart.

Q. What is your business?

A. Teacher.

Q. Are you man or woman?

A. Woman.

No explanation of this answer was found. Laton assumed throughout the character of a man.

Q. Are you alive or dead?

A. Dead.

Q. Where did you live?

A. Illinois.

Q. In what town?

A. Chicago.

Q. When did you die?

A. 1883.

Then followed many questions, first relating to the bill of fare of Henry W.'s dinners for one, two, and three weeks back. Laton could give the *menu* somewhat correctly for two weeks back, but beyond that he said "I don't know." His memory of them seemed somewhat but not greatly superior to Henry W.'s. Various problems in mental arithmetic were given, the simplest being 16×9 . The answers were always promptly writ-

ten and were uniformly wrong. Tested upon the dates of well-known historical events, his answers were all incorrect. Asked about my mother's name he wrote 'Mary Peters,' but afterward changed it to 'Lucy Williams,' both wholly wrong. My sisters' names were given as 'Winnifred,' 'Jennie,' and 'Carrie'—all wrong.

Q. Have you supernatural knowledge, or do you just guess?

A. Sometimes guess, but often spirit knows. Sometimes he will lie.

The next sitting was held two days later.

Q. Who is writing?

A. Bart Laton.

Q. Who was mayor of Chicago when you died?

A. Harrison. [Carter Harrison was mayor of Chicago from 1879 to 1887.]

Q. How long did you live in Chicago?

A. Twenty years.

Q. You must be well acquainted with the city.

A. Yes.

Q. Begin with Michigan avenue and name the streets west.

A. Michigan, Wabash, State, Clark, (hesitates)—forget.

Henry W. is then asked to name the streets, and can name only Michigan, Clark and State.

Q. Now your name is not Bart Laton at all. Your name is Frank Sabine, and you lived in St. Louis, and you died November 16, 1843. Now, who are you?

A. Frank Sabine.

Q. Where did you live?

A. St. Louis.

Q. When did you die?

A. September 14, 1847.

Q. What was your business in St. Louis?

A. Banker.

Q. How many thousand dollars were you worth?

A. 750,000.

Q. Can you tell us something which Henry W. doesn't know?

A. Perhaps. I'm not a fraud.

Q. Who was mayor of St. Louis when you died?

A. John Williams.

At the next sitting, a week later, Henry W.'s father and mother, who were visiting him, were present, and a young lady named Miss J.

Q. Who is it that is writing?

A. Bart Laton.

Q. Where did you live?

A. Chicago.

Q. When were you born ?

A. 1845.

Q. How old are you ?

A. 50. [This sitting was held in 1895.]

In this and other answers where easy computations are correctly made, there is a slight hesitation accompanied by muscular indication of effort in the arm.

Q. Where are you now ?

A. Here.

Q. But I don't see you.

A. Spirit.

Q. Well, where are you as a spirit ?

A. In me, the writer.

Q. Multiply 23 by 22.

A. 3546.

Q. That was wrong; how do you explain your answer ?

A. Guessed.

Q. Now, the other day you represented that you were some one else. Who was it ?

A. Stephen Langdon.

Q. Where from ?

A. St. Louis.

Q. When did you die ?

A. 1846.

My question was in the form of a suggestion that he, the writer, is Stephen Langdon, which is naïvely accepted.

Q. What was your occupation ?

A. Banker.

Q. But who was Frank Sabine ?

A. I had the name wrong. His name was Frank Sabine.

Q. Now I want to know how you happened to take the name Laton ?

A. My father's name.

Q. But where did the name Laton come from ? Where did Henry W. ever hear it ?

A. Not Henry W. but my father.

Q. (By Miss J.) Have you any message for any of us ?

A. I don't know you well enough, but Prof. P—— should not be so incredulous about spiritualism.

According to Laton's later account of himself he was a tutor in a family in Chicago before the Civil War, where Henry W.'s father was a chore boy in the same family. Altogether inconsistent with this is his present statement that he doesn't know any of the company well enough to give them a message.

Q. But tell me how you came to assume the name Laton ?

A. *I am a spirit.* (Written with great energy as heavily as the pencil would write.)

Q. What is your relation to Henry W.?

A. I am a spirit, and control Henry W.

Q. Of all the spirits why did you come to control Henry W.?

A. I was near when he began to develop.

Q. Now look here, this is nonsense. You are not a spirit, and you know you are not, and I must know how you came to pick up the name Laton.

A. Darn you, I am Laton.

Henry W. is allowed to read this, and, his father and mother being present, is greatly vexed and asks, "Did I write that?"

After this sitting Henry W. was absent for two years. During this time he never tried automatic writing, was never hypnotized, and apparently gave no thought to the previous experiments. The sittings were renewed in the spring of 1897.

Q. Who are you?

A. Bart Layton. [Note change of spelling from this on.]

Q. What have you to say to us?

A. Glad to see you.

Q. When did you write for us before? Give year, month and day.

A. I don't know.

Q. In what year was it?

A. 1895.

Q. In what month?

A. Don't know. April, I remember. [It was June, 1895.]

Q. Tell us more about yourself.

A. I lived in Chicago.

Q. Do you live there still?

A. I am here now.

Q. How long did you live in Chicago?

A. Twenty years.

Q. Why did you leave there?

A. None of your business.

Q. In what year did you leave?

A. 1872.

Q. What was your occupation?

A. Doctor and carpenter.

Q. In what year were you born?

A. 1840.

Q. In what year did you die?

A. Did who die?

Q. In what year did you pass out?

A. 1875.

Q. Who was Stephen Langdon?

A. Chicago friend.

Q. Did you write Chicago friend?

A. Yes, can't you read?

Q. How many minutes was it before you brought the book?

A. Five. (After hesitation.)

This question relates to the post-hypnotic experiment tried upon Henry W. two years before and related above. It was sprung upon the communicator to test his relationship with Henry W.'s hypnotic personality. The answers to the questions following about the fence are still more striking, for Henry W. never knew anything at all about the fence episode, having been tested after the experiment two years before.

Q. Where did you get the book?

A. Table.

Q. What did you do with it?

A. Gave it to you.

Q. Who else was with us?

A. Mr. Grimes. [Correct.]

Q. What was it you had to step over?

A. Fence.

Q. What kind?

A. Barb wire.

Q. Who was it who stepped over the fence?

A. I did, you fool.

Q. What was your name?

A. Bart Layton.

The following questions and answers were from the last two sittings held two and three weeks later. At the beginning, an attempt, not very successful, was made to cultivate a good humor in the communicator. At the end, a second successful attempt was made to anger him.

Q. Who is writing?

A. Bart Layton.

Q. Good morning, Mr. Laton. Glad to see you. Would like to get better acquainted with you.

A. I don't care.

Q. Now, Mr. Laton, will you give us some message if you will be so kind?

A. From whom?

Q. Well, from yourself.

A. I am all right.

Q. From whom could you bring us a message?

A. Whom do you know?

Q. Well, I have many friends. Are you in communication with my friends?

A. George White.

In all Laton's writings this was the one single instance of the brilliantly intuitive type, though not a very striking one. I had an uncle named George White for whom I was named and who was killed in the Civil War. Henry W. knew nothing of

this, but he had had opportunities of seeing my own name written in full, containing these two names with a third name, however, Thomas, between them. In answer to further questions, Laton said that George White was my father or grandfather and 'passed out naturally' fifteen years ago. Upon a request for a message from George White, he wrote, He is glad to see you so well.

Q. Tell us, Mr. Laton, something we don't know, won't you?

A. Think you're smart, don't you?

Q. When did you write for us before?

A. Five weeks ago.

Q. Where have you been in the meantime?

A. Everywhere.

Q. Tell us something of your own life. How do you pass your time every day?

A. I never entirely leave Henry W., but partly so.

Q. When you leave him where do you go?

A. Anywhere or nowhere.

Q. What were you doing yesterday at this time?

A. With Henry W.

Q. What did you have for supper Thursday of this week?

A. None of your business.

Then followed questions in mental arithmetic in which my assistant and I both thought attentively of a certain incorrect answer. Wrong answers were given in each case, but not the ones we thought of. Laton was also asked to give the time of day, which in each case he gave incorrectly, even when we were looking intently at our watches.

Q. What was Mr. Laton's occupation in Chicago?

A. Carpenter.

Q. Two years ago you said he was a teacher.

A. Well, he—I used to be a teacher.

Q. Do you dance?

A. We don't dance who have passed out.

Q. Why don't you who have passed out dance?

A. You can't understand; we are only as you would say partly material.

Q. When you get through writing to-day, where is the part that is not material going?

A. It goes nowhere or anywhere as you choose to know space.

Q. Do you ride a bicycle?

A. Only through Henry W.

Q. Two years ago you spelled your name 'Laton.' How do you account for that?

A. Too many Latons; like the other better.

Q. I think you are an unmitigated fraud. What have you to say to that?

A. Shut up, you poor old idiot. Think I most always answer your damned old questions right? I can lie to you whenever I damned please.

This answer was accompanied by great muscular excitement of the hand and arm. There being one or two illegible words, I had the communicator repeat parts of the answer several times. The word 'danned,' evidently intended for 'damned,' was so spelled each time. Henry W., meanwhile, was calmly reading and never knew what had been written.

The automatic writing was now discontinued, as evidently there was little more to be gained from Laton. But the familiarity of the communicator with the hypnotic actions of Henry W. suggested one further experiment. If Henry W. were hypnotized, would the hypnotic personality assume the name Laton, and give the same account of himself orally? Henry W. consenting, hypnosis was induced by a few suggestions and was tested by a simple experiment in hallucination. I suggested that there was a five-dollar gold piece on the edge of the table. The subject saw it and asked whose it was. My assistant jokingly said that it must be Laton's, whereupon the subject went through the motions of grabbing it and putting it in his pocket with great glee, remarking, "If it's Laton's, it is mine, for he is a part of me." Evidently, then, the hypnotic personality did not necessarily consider itself as Laton, but my assistant's remark was perhaps a suggestion that Laton was not present. I therefore changed the subject's seat, bade him close his eyes for a moment and suggested that he was Laton. This was instantly successful, and a free conversation was then carried on with Laton as long as I wished. The subject's eyes were wide open and his manner easy and unconstrained, though not quite that of Henry W. There was no sign of Laton's recent anger, but the account that he gave of himself was the same as given in writing, with some added details. He said that he 'died' in 1875 at the age of sixty, that he lived on North Clark Street, that he was before the war a tutor in the family of Mr. Pullman, where Henry W.'s father was then a chore-boy, that he was a tutor of Mr. Pullman's little girl, but failing in the capacity of a teacher, and Chicago building up rapidly, he went to carpenter-

ing. He said further that he had been with Henry W. since '75 [95?], that he had chosen him because he was the right kind. "He developed," he said, "and I got a chance to show myself." A few other questions were asked testing the power of thought-transference, but without result. The subject was then awakened and found to have no knowledge of what had happened. A striking feature of the experiment was the instantaneous and naïve assumption of the personality of Laton after the suggestion was made. As soon as the word was spoken, there was no confusion of 'he' and 'I' as relating respectively to Henry W. and to Laton.

Before commenting upon any peculiarities of the secondary personality indicated by the above conversations, I may mention some other details of the investigation. As may be seen, my attempts to trace from internal evidence the origin of the name, Bart Laton, were not successful. The external evidence yielded no better results. I could not learn that Henry W. or any member of his family had ever known any one bearing the name Bart Laton, or even Laton. The hypothesis that there was a real Bart Laton whose 'spirit' was communicating through Henry W. will hardly appeal to any one who has read the questions and answers, even if we grant, with Dr. Hodgson, that communicating 'spirits' must *a priori* be suffering from a certain amount of 'confusion,' or even 'aphasia' and 'agraphia.' The frequent contradictions as to the time of his birth and death, his uncertainty as to whether he was a teacher, carpenter or doctor, his willingness to resign his personality in favor of Frank Sabine or Stephen Langdon, together with the unmistakable evidence that the whole 'history' was progressively constructed in answer to my questions, make such a view as improbable as it is unnecessary. I did not, however, omit to make diligent inquiries in Chicago. The experiments were completed before Mr. Pullman's death, and through the kindness of Hon. Frank Lowden, his son-in-law, I learned that none of Mr. Pullman's family had known any one bearing the name Bart Laton, that Mr. Pullman's daughter had never had a tutor by that name or any other male tutor. The chronology given by the communicator would in any case make such a relation impossible.

The communicator's statement that Henry W.'s father was at one time a chore-boy in Mr. Pullman's family was correct, but this was known by Henry W. and may indeed have served as a basis for the communicator's romance. I concluded, therefore, that the origin of the name is to be traced directly to the constructive imagination of the secondary personality.

In attempting any description of the marks of the secondary personality, either from a study of this or of other cases of automatism, we are struck perhaps first of all by the remarkable activity of the constructive imagination. Quite independent of all theories, the presence of this particular form of mental activity is characteristic. It is shown in this case throughout the whole conversation, for instance, in the fictitious answers to the mathematical problems, in the construction of the Chicago 'history,' and in the invention of the names, Mary Peters, Lucy Williams, Stephen Langdon, John Williams, etc. Frank Sabine differs from the others only in this, that I invented it myself and suggested it to the communicator. By way of experiment, any number of such names, some commonplace like John Williams, others more unique like Bart Laton, may be collected by any one who will ask a number of his friends to assume or invent a name on the spur of the moment. If, for the sake of the argument, we omit the comparatively few instances of the brilliantly intuitive type, the great mass of automatic utterances in this and in all other reported cases reveals the activity of the constructive imagination and shows further the most rigid adherence to the law of limitation to the store of memory images possessed by the subject. This limitation is painfully apparent in the utterances of my subject. The communicator has a vivid imagination, but the materials are all drawn from the experience of Henry W. The hypermnesia exhibited by many subjects and shown in a very trifling degree by mine—as, for instance, when Laton mentions one more of the Chicago streets than Henry W. can—in no way, of course, violates this law.

The suggestibility of the secondary personality is also apparent from this case. The communicator is willing, in response to my suggestion, to change his whole personality, and

become Frank Sabine of St. Louis, and then proceeds to construct a 'history' consistent with the suggestion. In response to my suggestion again, he accepts the name Stephen Langdon, at another time becomes a good penman, admits that he 'guessed' the answers, etc. His suggestibility is limited only by a sort of insistent idea that he is a 'spirit,' which determines the answers in the form of a 'spirit' personality limited to the scant knowledge of what such a personality should be, possessed by Henry W. The very opposition which he shows in the later sittings is apparently the result of my indirect suggestion of hostility shown by the skeptical and disrespectful attitude which I assumed. In this connection, it is worthy of notice that in any conversation with a secondary personality, the questions themselves form a series of suggestions, and that properly prepared questions are of first importance. In the present instance, my questions may have determined the whole 'history' of Laton, and a different set of questions would have resulted perhaps in a totally different account. My first question, Who are you? really suggests a doubling of the personality. My question, Are you alive or dead? suggests perhaps the 'spirit' idea. The questions were well adapted to the study of the birth and development of a 'spirit' personality, but it would be interesting to know what a wholly different set of questions would have produced. For instance, the first question might have been, not, Who are you? but, Write your name in vertical script. If then the communicator had given the name, Bart Laton, I might merely have expressed surprise that his name was not Henry W., thus avoiding any even remote suggestion of a 'spirit' presence.

Another peculiarity of the secondary personality which has been noticed in other cases is its rather low or 'common' moral and intellectual tone. This was conspicuous with Laton as well as with the other communicators mentioned in this paper. In the case of Laton, my skeptical attitude was assumed for the purpose of allowing this trait to develop and to see what kind of language the communicator would use when angered. Stupid profanity was the result. The answers throughout were commonplace. When asked for a message

from the 'spirit' of my uncle, he can only say "He is glad to see you so well." This peculiar trait is strikingly illustrated in one of my other subjects, the young girl mentioned above. To test her alleged clairvoyant powers, I had prepared a name written upon a sheet of paper and sealed in an opaque envelope. The communicator, the 'spirit' of the girl's deceased mother, professed to be able to read it and said that it was 'Mamie Nolds.' This was wholly incorrect, and I so stated. The communicator, however, insisted and insisted again that the name was 'Mamie Nolds.' I therefore opened the envelope, held up the writing, and triumphantly asked, "Now what have you to say?" To which this interesting and characteristic answer was written, "I think you are furrucht in the kopf," misspelled school-girl slang of rather a low order, such as I think the subject herself would not have used even with her associates. The utterances are sometimes of a flippant tone. One of the 'controls' of the girl just mentioned, professing to be the 'spirit' of 'Ben Adams,' who passed away in 1872, always wrote flippant answers. For instance, his veracity being questioned, he wrote, "I am not a fraud or a frog either." Asked the day and month of his death, he said, "I don't know. I got hit on the head."

Among the peculiarities of the secondary personality we may, perhaps, regard as fourth in order the brilliantly intuitive character of a very limited number of these utterances. In the case described by Dr. Hodgson these are very striking. With my subjects I have mentioned two instances of such utterances. Even with Bart Laton there is, as it were, a trace of the presence of such a trait in his mention of George White. Considering the sluggish character of Laton's mind and his very slight ability to use the latent memories of Henry W., it does not seem very probable to me that Laton was shrewdly using a latent memory of a part of my name, hoping that it might happen to coincide with the name of some deceased relative. Such an explanation is possible, or it may have been a chance guess, but, considering the large number of such cases which the history of automatism affords, it seems to me better to note this power of happy intuition as one of the marks of the secondary personality. The explanation of it is not within the purpose

of the present paper. It seems like the flickering survival of some ancient faculty. One thing only is sure in this case, the origin of the utterance was with the immediate participants in the experiment. For, let us suppose that it was not a guess nor the revival of a latent memory of Henry W., but that it was communicated from some outside source. We should have to choose then between its being communicated unconsciously by me and its being communicated by the 'spirit' of the deceased George White. Put in this form, the 'spirit' hypothesis immediately becomes absurd, for, even if we have to assume, as is not indeed really necessary, that the name was communicated 'telepathically' by me, we must assume that and a great deal more if it was communicated by George White. Furthermore, if I may risk taxing the patience of the reader by further reasons where none are necessary, it would be more probable that the suggestion came from me from the fact that I have always had a romantic interest in the memory of this uncle, while George White, himself, hardly knew me at all. To my mind, however, rejecting the 'spirit' hypothesis does not mean accepting that of 'telepathy.' When the characteristics of the secondary personality become subject to accurate scientific description, some other hypothesis may be found quite apart from either.

Meanwhile it seems to me of the highest importance that the dignity of psychological science should be maintained by the use of modern logical methods. For instance, it seems to be regarded as a 'test' of the 'spirit' hypothesis by those who have urged it and to be naïvely accepted as such by reviewers and critics, when the communicator is able to relate that which is occurring at a distant place. The instances of this seem always to have some element of uncertainty about them. But granting that such uncertainty were removed, what then? Eliminating fraud and telepathy from those present, it is argued that it must then be 'spirits.' Imagine such methods pursued now in the physical sciences! Any new manifestations or reaction not following known laws might be attributed to 'spirits'! For instance, light-rays do not penetrate opaque substances. The new X-rays ignore this law; they must be due to 'spirits.'

But, it may be said, we have, in the case reported by Dr. Hodgson, other tests of quite a different kind, where only the 'spirit' hypothesis is applicable. The one which appears to be particularly convincing is that of a communicator who gives as his name that of a New York man known to have died some time before, and who offers various convincing proofs of his identity. But is the logical aspect of this kind of evidence any better? Again, supposing that fraud and telepathy from those present are eliminated (and from the published reports, fraud at least seems to have been conscientiously eliminated), the bare facts of the case are that a certain woman in the city of Boston, in a certain abnormal condition, writes or relates occurrences which happened not only at a distant place, but at a past time, and shows herself familiar with certain friends and doings of the New York man. This is more 'remarkable' even than light-rays piercing opaque substances. Surely it must be due to 'spirits'! It may be that science will ultimately gain such knowledge of disembodied minds that it can use them as the basis of an explanation of phenomena in abnormal psychology, but at present the advancing of such hypotheses by psychologists can only serve to further the cause of superstition, to which people are already only too willing to fly when something mysterious presents itself.

As regards the various traits of the secondary personality, some of which have been referred to in this paper, it has been suggested by Mr. Podmore and others that they are instances of survival or reversion. One cannot indeed fail to be impressed by the similarity of these traits to what we know or conjecture about the primitive mind. The general low moral and intellectual tone of the communications, the vulgarity and mild profanity, the frequent impersonation of the medicine-man, Quaker doctor, Indian doctor, etc., the keen memory and dull reason, the vivid constructive imagination, the deception and prevarication, the unwavering belief in spiritism, and the superstitious devotion to amulets, trinkets, and petty articles of ornament or apparel, all point to an early stage in the evolution of mind. Even the peculiar intuitive power sometimes exhibited by the secondary personality may be compared to the superior

intuition of woman, whose mental peculiarities are in general representative of the more stable, basal and abiding phenomena of mind. Both may point to some nearly extinct faculty no longer serviceable. Still other peculiarities suggest the same theory, such as the extreme suggestibility and motor force of ideas, marks of automatism and of the hypnotic state, and at the same time characteristic of the child and savage mind. In close relation to this is the peculiar intimate connection between ideas and organic, nutritive and circulatory processes, best shown in hypnosis, and common to this group of phenomena. In view of such facts as these, certain of the more simple physiological theories of double personality gain considerable plausibility, such, for instance, as the revival of disused and outgrown brain tracts, particularly perhaps those of the less specialized hemisphere. The frequent appearance in automatic writing of *Spiegelschrift*, which occurs also among children, lends some support to this view.

It has not, however, been my purpose in this paper to propose any new theory or establish any old one to account for the phenomena of automatism, but rather to urge the extension of experimental inquiries in this direction, to point out certain prevailing peculiarities of the secondary personality, and to insist that the more complex and mysterious cases are to be understood by a constant reference to the simpler ones.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

AN INVESTIGATION OF CERTAIN FACTORS AFFECTING THE RELATIONS OF DERMAL AND OPTICAL SPACE.¹

By JAMES ROWLAND ANGELL, JESSIE N. SPRAY, E. W. MAHOOD.

The experiments reported in this paper deal with two problems touching the relation of dermal and optical space. (1) What effect does the absolute weight of the dermal stimulus exercise upon the comparison of linear extents sensed by the skin with linear extents seen? (2) What effect does the temperature of the dermal stimulus exercise upon the same comparison? The dermal space judgments are all based on cutaneous as distinct from tactual stimulations, *i. e.*, they involve passive and not active pressure. The optical judgments rest on direct vision. No attempt was made to exclude eye movements.

Before passing on to a description of apparatus and method we may profitably pass in review very briefly some of the experimental observations upon matters immediately cognate to our inquiries.²

Numerous investigations have been made upon the sensitiveness of various areas of the skin to stimulations with two points,

¹The experiments described were actually executed by the two persons whose names appear with my own at the head of the article. They deserve whatever credit belongs to the careful, painstaking supervision of the work, the preparation of apparatus and the construction of tabular reports. I am responsible for any shortcomings in the general method employed and in the form of presentation of results.

J. R. A.

²The very complete bibliography in M. Henri's scholarly monograph 'Ueber die Raumwahrnehmungen des Tastsinnes' renders any extended reference to the literature of the subject superfluous. We may preface our statement by saying that in none of the literature to which we have had access is there any account of investigations upon our immediate problems.

applied both simultaneously and successively.¹ The different parts of the body are found to vary very widely from one another. Vierordt's formulation, which is only roughly accurate, states that the limen is smaller the more mobile the part concerned.² Children and blind persons are more sensitive than normal adults. Bearing more closely on our work are the observations of certain investigators upon the effects of changes in pressure on the limen for double-point discrimination. The results are not in agreement with each other, some reporting no effect following change of pressure, others finding the limen smallest when the stimulations are relatively intense. Henri reports a variation in effects of pressure as dependent upon the special area selected.³ When the points are of different temperatures, the limen falls. When both points are cold, the discrimination is more delicate than when both are warm, but the best results occur when the points are at the skin temperature.⁴ On the arm the limen is smaller in the transverse than in the longitudinal direction. The more delicate the discriminative capacities of any area of the skin, the greater is the apparent distance between points stimulated upon this area.⁵ The distance between two points on the skin is usually underestimated. Wundt found that this underestimation decreased with increasing pressure, but he gives no detailed statements of the observations and his method seems to have involved the adjusting of dividers by his subject in a peculiar manner.⁶ The apparent distance between two dermal point stimuli is decreased if they follow stimuli nearer together, and increased if they follow stimuli further apart. That is to say, cutaneous experiences of this character are subject to effects of contrast. The distance between two points on the skin is felt as greater than the length of a line objectively equal to it.⁷ Both in the judgment of cutaneous space

¹Judd, *Phil. Studien*, 1896.

²Vierordt, *Zeit. für Biologie*, 1870.

³Raumwahrnehmungen des Tastsinnes.

⁴Goldscheider, Du Bois-Reymond, *Archiv*, 1885. Klug, *Wärmerortsinn, Arbeiten d. physiol. Anstalt*, Leipzig, 1876.

⁵Fechner, *Abhandl. Sachs. Gesell. d. Wiss.*, Leipzig, 1884.

⁶Wundt, *Theorie d. Sinneswahrnehmung*.

⁷Nichols, *Our Notions of Number and Space*. Parrish, *Amer. Journ. Psychol.*, VI. and VIII.

and in the localizing of cutaneous stimulations, it is probable that with many persons visual imagery plays a conspicuous part.¹ When compared with visual extents, stimuli involving tactile and articular elements are regularly underestimated.² These seem to be the points most definitely established, which concern more or less directly our problems and results.

APPARATUS AND METHOD.

For giving the cutaneous linear stimulations a series of cards of various lengths was employed. Those used in the tests involving temperature were made of thin brass, while those employed in the other tests were of stiff rubber. These cards were, by means of a simple device, clamped into one of Professor Jastrow's æsthesiometers, provided with a cup at the top for the reception of weights. This instrument is so constructed as to insure equal pressure over the area stimulated and is too well known to require further description.

The cards ranged from $\frac{1}{2}$ cm. to 10 cm. in length, each differing from the one next it by $\frac{1}{2}$ cm., with the exception of those between 1 cm. and 2 cm. These differed from one another by $\frac{1}{4}$ cm. These lengths were determined by the results of preliminary tests of investigation and control. The shortest length selected was that at which under all the conditions of the experimentation the stimulus was felt as linear and not merely punctual. The longest length was determined by the possibility of applying the stimulus evenly throughout its whole length. The increase in the number of steps between 1 cm. and 2 cm. results in somewhat richer data over this part of the scale. It has no other significance. Each card was 1.75 mm. thick.

The brass and rubber cards were exact duplicates of each other and the utmost care was exercised to secure smooth, even surfaces of contact. Moreover, all the cards were of precisely the same weight. This permitted us in the tests with changing temperatures to keep the weight of the stimuli objectively equal,

¹ Washburn, *Phil. Studien*, 1895.

² Henri, Op. cit. Jastrow, *Mind*, XI.; *Am. Journ. Psy.*, III.

whether a long line or a short line was being used. We did not attempt to secure subjective equality of pressures. We were also enabled by means of this arrangement to make all changes of weight, in the tests where the temperature was kept constant, directly upon the *æsthesiometer* and without regard to the length of the stimulus being employed.

The different degrees of pressure were obtained by adding or removing weights from the cup of the *æsthesiometer*. Inasmuch as our problem concerns the effect of increase and decrease of pressure upon the linear judgments of the skin, we have confined most of our experiments to ranges where the stimulus produces a relatively clear and distinct impression, avoiding on the one hand such stimulations as are very close to the pain limen, and on the other hand those which lie so near the pressure limen as to be hopelessly vacillating and vague. We have made a limited number of tests, however, covering both points. The weights used (this is the total weight of the apparatus as it rested on the skin) were as follows: 31.47 g., 35.47 g., 39.47 g., 43.47 g., 47.47 g., 51.47 g., 55.47 g. It will be seen that there are three weights above and three below 43.47 g. Each weight differs from the next above by 4 g. This increment was selected because it was found to involve a just easily noticeable difference, when attention was directed to the pressure. The weight 43.47 g. is referred to in the following pages as the 'normal,' because the effects of the lighter and heavier weights are compared with it as a standard.

The thermal changes were secured by heating and cooling the brass cards. Three different degrees of cold were used, viz., 9°, 13°, 18° Centigrade, and three degrees of heat, viz., 40°, 45°, 50°. Judgments based on these compound thermal-pressure stimulations are compared with those based on the thermally indifferent stimuli, *i. e.*, those at the physiological zero. It is for several reasons exceedingly difficult to obtain linear stimulations which are absolutely indifferent thermally. Among other reasons may be mentioned the sensitiveness to mechanical stimuli of the neural apparatus for temperature. In the appreciable interval between the removal of the stimulus

card from the temperature chamber and its application to the skin, there is necessarily a slight change in its temperature, which makes the record of our tests upon this subject less accurate, absolutely considered, than are those upon pressure. As representing, however, the direction and general nature of the changes produced by the introduction of thermal elements, we believe the results are thoroughly reliable. The particular temperatures adopted were selected as representing the change from the physiological zero, through mild but distinct thermal sensations, up to those whose intensity renders the temperature portion of the experience distinctly paramount to the pressure portion.

The optical stimuli consisted of black, horizontal lines drawn exactly parallel with one another on a large white card, 55 cm. by 35 cm., with a black margin 2 cm. in width. A space of .5 cm. was left between each pair of lines, so that each line was seen clearly by itself. The lines were arranged in the order of length, beginning with a mere dot and ending with a line 12 cm. long. This procedure practically eliminates the effects of contrast, as each line is midway in length between its two neighbors. The only exceptions to this statement are the two extremes, which are as a matter of fact of no importance, for reasons which will appear shortly. Each line differed from the one next it by .125 cm. It will be observed that this approximates the just noticeable difference of visual extent for lines in the median portion of the scale, being somewhat too large for the shorter lines and too small for the longer ones. The scale might easily have been made to conform to the law for such judgments, but it did not appear that any material gain would accrue for the present purpose. Each of the dermal cards corresponded exactly in length to one of these visual lines. There were, however, 97 visual lengths and only 22 dermal lengths. Except in the range from 1 cm. to 2 cm. there were three visual lines intermediate in length between any two consecutive dermal lines; from 1 cm. to 2 cm. there was only one intermediate line. But it will be remembered that in that portion of the scale the dermal stimuli are more numerous.

In the present experiments the cutaneous stimuli were ap-

plied to one area only. Starting at a point on the volar side of the forearm, about 15 mm. above the wrist joint, a tract was mapped out a trifle less than 4 mm. wide, extending up the long axis of the arm toward the elbow. One extremity of the dermal cards was always placed upon this starting-point, so that the area stimulated was kept as constant as possible. The most comfortable position of the arm and body was of course sought, in order that fatigue might not enter as a disturbing element. To insure this result still further, the morning hours from 8.30 to 10 were employed for the work.

An illustration will now make clear the actual procedure in any particular test. The subject sat with closed eyes just before a table upon which, at a convenient distance, was placed the chart of visual lines. The operator gave a verbal signal 'ready' and a moment later applied the dermal stimulus to the subject's arm, allowing it to remain in place for somewhat less than two seconds, which was found by experiment to be the most favorable time for its clear perception. The stimulus was then removed; the subject opened his eyes and selected from the visual lines the one judged equal in length to the dermal stimulus. It will be seen that this is essentially an application to the present problems of the method of average error.

We venture to emphasize once again (1) that only one area of the skin is concerned in this report; (2) that only stimulations parallel with the long axis of the limb are involved; (3) that only horizontal visual lines are concerned, and (4) that only such judgments are at issue as consist in comparing a previously experienced dermal stimulus with a sequent visual stimulus, never the reverse. The probable bearing of certain of these limitations on the general interpretative value of our results will be canvassed later on in the paper. Suffice it to say that certain of these matters are to be investigated more fully. Meantime it may be added that, by limiting the field in this way, we are enabled, within the bounds indicated, to put a larger degree of confidence in our tests, which number upwards of 6,500, than would otherwise have been the case.

The experiments were made upon two persons, one serving as subject for the temperature tests, the other for the pressure

tests. Slightly less tests were made on temperature than on pressure. With both subjects, as has been mentioned, preliminary practice tests were made with constant pressure and temperature. These are neglected in the tables and curves. It is a matter of regret that tests could not be made on more persons.

In order to minimize the possible effects of expectation the subject was kept in ignorance of the order in which the stimuli were given. As a means of baffling speculation on the part of the subject regarding this order, and also as a means of equalizing any effects of dermal contrast, the procedure was varied in a number of ways. Sometimes the shorter lines were employed first and then the lengths gradually increased. Sometimes the reverse order was followed. Again the two methods were combined with various modifications, and finally some of the tests were made with utter disregard of any regular order. As a matter of fact the subject soon ceased to pay any special attention to the order, and the unconscious effects of contrast are quite certainly reduced to a negligible quantity, if not, indeed, wholly eliminated. A similar method was employed regarding the thermal and pressure variations, each one of which necessarily involves all the lengths of lines. In order that the effects of practice might be evenly distributed over the judgments involving such changes from the normal pressure and temperature, care was exercised to alternate the tests every two or three days. Accordingly no one length of line and no one degree of temperature or pressure is at any material advantage as compared with any other. The tables and curves are computed on the basis of the averages for the whole period of experimentation under such equalizing conditions, except when explicitly otherwise stated. Thus, for example, upon days when temperature or pressure tests were made involving departure from the normals, there would always be a series of normal tests at the outset and another series at the close of the sitting. In order to avoid confusion and vacillation in the judgments, only one variation from the normal, either as regards pressure or temperature, was ever made upon the same day.

Attention was called at the outset to the fact that only the long axis of the arm was used in making the cutaneous stimula-

tions. That a difference exists in the spatial judgments, which arise when a stimulus is applied in different directions to the forearm, is of course well known. This fact, like the difference in the space judgments arising from cutaneous areas more widely separated, makes it impossible to employ our results as representative of other cutaneous surfaces, although it does not seem probable that the direction of the changes noticed would be materially modified. If, as Henri states, pressure affects judgments of point distances very differently for different cutaneous localities, then our experiments dealing with lines are probably significant only for the special area investigated.

Similarly we must limit our statements on the side of the visual lines to such as are horizontal. But inasmuch as vertical lines are generally overestimated when compared with horizontal lines, it will be seen that our procedure will tend to diminish rather than to magnify the disparities between the cutaneous and visual extents.

It is hoped that experiments may be carried out reversing the procedure in these and comparing, by the method of right and wrong cases, visual extents with sequent cutaneous extents.

Results.

I. JUDGMENTS BASED ON VARIABLE PRESSURES.

The curves in figures 1 and 2 furnish a graphic presentation of the judgments under varying pressures of the dermal stimulus. It will be noticed that at the 'normal' pressure, *i. e.*, 43.47 g., the very short cutaneous lines varying from .5 cm. to 1.25 cm. are overestimated when compared with visual lines; from that point on, however, all the cutaneous lines are underestimated. This underestimation is relatively greatest between the lengths 2 cm. and 3.5 cm. and is only in slight measure exceeded absolutely by the underestimation of lengths from 7.5 cm. to 8.5 cm. Elsewhere in the scale there are relatively small wave-like fluctuations.

Just what may be the precise grounds, physiological and psychological, of the differences in the space perceptions of disparate senses we have at present no intimation; granted, how-

ever, a tendency, as in this instance, to underestimate cutaneous linear space, when compared with visual linear space, we might anticipate that an increase in the pressure of the cutaneous stimulus would, by bringing out more clearly unnoticed dermal sensations, tend to increase the felt size of the object and so reduce

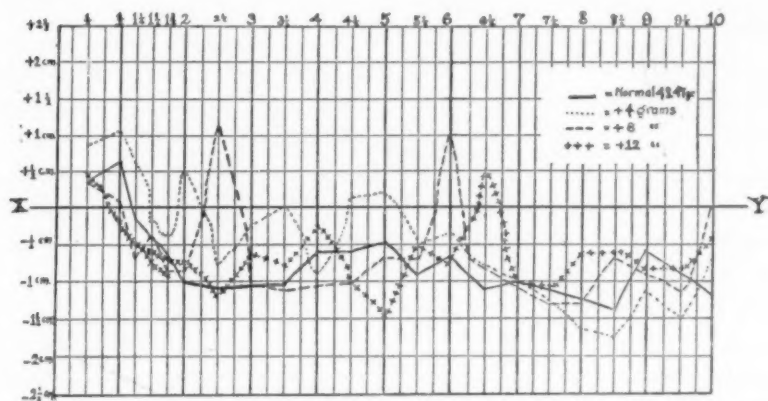


FIG. 1. Showing the average error in cm. of the judgments for the different lengths of apparatus and the direction of the error for the different pressures above the normal. The numbers on the line *XY* indicate the lengths of the cutaneous stimuli. Where the curves run below the line, underestimation is indicated; *i. e.*, a visual line shorter than the cutaneous line is selected as being equal to it. Overestimations are represented by points on the curve above the line *XY*. The horizontal lines above and below *XY* represent .5 cm.

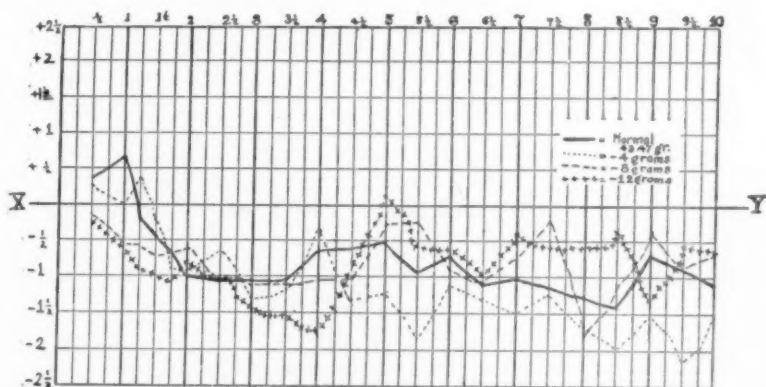


FIG. 2. Showing the average error in cm. of the judgments for the different lengths of apparatus and the direction of the error for different pressures below the normal. For further explanations see Fig. 1.

the underestimation. A point could then be determined where the underestimation was at a maximum. Above this, every increase in pressure, up to the point of annoying intensity, should be accompanied by decrease—or, indeed, a possible reversal—of the error. We should, moreover, expect the average variation of successive judgments to grow greater toward both extremes. This was, in point of fact, what we anticipated, and is, with some unforeseen modifications, what the results show.

The general principle is illustrated in the judgments upon the lines shorter than 1.25 cm. Here, oddly enough, the usual relation is inverted and the normal cutaneous line is overestimated. The increase of 4 g. results in still further overestimation, as we should expect, but the increase of 8 g. and 12 g. does not add to it. This is a symptom of what the curves show all through, *i. e.*, that the heavier weights produce irregularities and relatively large variations in accordance with our expectations. Not until the 35.47 g. weight is employed do we get a reversal and a consequent underestimation. In the ranges from 1.5 cm. to 5 cm. all four of the lightest weights are underestimated. The general effect of increasing the pressure over this part of the scale is to reduce the error. Two of the heavier weights produce occasionally an overestimation, but irregularity is again a feature of their results. Between 5.5 cm. and 6.5 cm. we appear to have a turning-point. The two heaviest weights produce marked overestimations here, after which all the remaining lines are underestimated regardless of the pressure.

A series of tests was made, which was not extensive enough to incorporate in these curves, showing the effect of very heavy and very light stimuli. Tests with 143.47 g. produced no marked change in the results. Underestimation was still the prevalent error, with no noticeable alteration in the amount of the error as compared with the normal and with a fair percentage of correct judgments, *viz.*, 20%. Tests with weights of 20.27 g. and 9.2 g. resulted in large underestimations and great subjective difficulty in making the judgments. When, however, as in two series of tests, the lines were kept the same—the longest and shortest were used—while the widest varia-

tions possible were made in the pressure, the errors became few and small, although they still consisted of underestimations.

Summing up on these points, then, we may say: (1) That for all degrees of pressure the dominant tendency in comparing cutaneous linear stimuli from the volar surface of the forearm with horizontal visual lines is toward underestimation. (2) The amount of this underestimation decreases with increasing pressure up to certain limits and may at points give way to overestimation. (3) The degree of pressure productive of such overestimations and reductions in underestimations is not constant for different linear lengths. (4) Except for light pressures of 35 g. and under, there is a distinct tendency to overestimate lines shorter than 1 cm. In our tests this tendency reaches a maximum at 43.47 g., and at 143.47 g. has fallen to zero again.

TABLE I.¹

Average percentages for all lengths of apparatus.

	CORRECT JUDGMENTS.	UNDERESTIMA- TIONS.	OVERESTIMA- TIONS.
Normal Pressure.	15	67	18
Increasing Pressure. 4 grams.	14	56	30
Increasing Pressure. 8 grams.	14	55	31
Increasing Pressure. 12 grams.	12	63	25
Decreasing Pressure. 4 grams.	11	73	16
Decreasing Pressure. 8 grams.	12	69	19
Decreasing Pressure. 12 grams.	9	76	15

¹It is planned to reprint these studies at a later date in connection with other reports of experiments made in the University of Chicago laboratory. More complete tabular statements will be made at that time. The average variation is small, the figures for one subject, for example, grouping themselves under all conditions closely about 0.14 of the length of the stimulus.

Table I. presents the results in terms of tests underestimated, overestimated and correctly judged under the various conditions. This disregards the amount of such overestimation, but inasmuch as the amount of the errors is in part determined by the method employed, with its limited number of possible selections from among the visual lines, these tables are, perhaps, quite as representative as the curves.

As the pressures increase, the decrease in the regularity of the judgments, which was commented on a few lines back, is shown by the decreasing percentage of correct judgments. The general trend is toward a reduction of the number of underestimations, but the heaviest weight is less effective than either of the other two above the normal. The same decrease in percentage of correct judgments is noticeable in the case of decreasing pressures, and we meet with practical regularity in the increase of underestimates. The next to the lightest weight is, however, slightly less effective in this regard than its heavier neighbor.

TABLE II.

Showing effects of practice for three typical stimuli at normal pressure and temperature.

LENGTH OF STIMULUS.	ERROR AT BEGINNING.	ERROR AT CLOSE.	
.5 centimeter.	+ .99 — .15	— .2 .0	Subject A. Subject B.
4.5 centimeters.	— .65 — .43	— .04 — .2	Subject A. Subject B.
10 centimeters.	— 1.54 — .4	— .86 — .19	Subject A. Subject B.

Table II. shows the effects of practice. The results suggest that the disparity between cutaneous and visual linear space as we have it here is very deep-seated in the organism, for the unmistakable tendency is toward a permanent retention of the characteristic underestimation. This affects even the short lines generally overestimated.

II. JUDGMENTS BASED ON VARIABLE TEMPERATURES.

Figures 3 and 4 exhibit the effects of the variations of temperature upon the judgments under consideration. It will be

seen at a glance that the departures from the normal are much more marked than in the case of the pressure tests. As in the oft-quoted instance of apparent increase in size and weight of

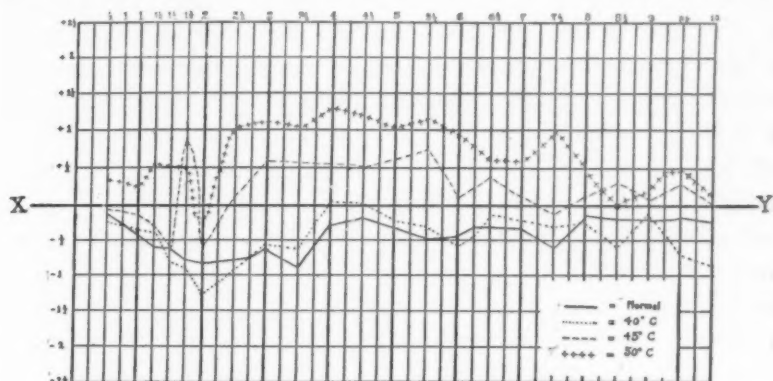


FIG. 3. Showing the average error in cm. of the judgments for the different lengths of apparatus and the direction of the error for the different temperatures above the normal. For further explanation see Fig. 1.

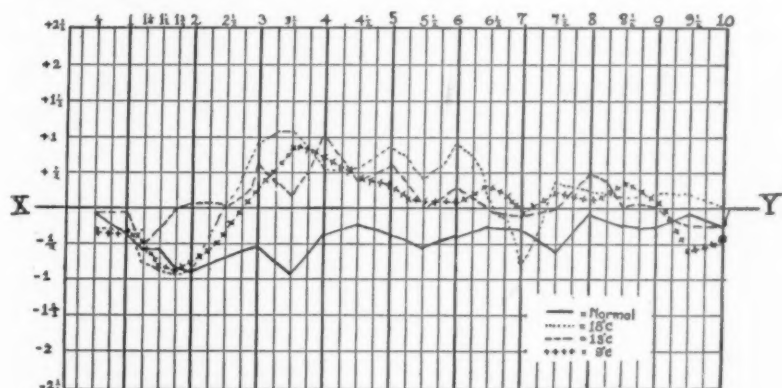


FIG. 4. Showing the average error in cm. of the judgments for the different lengths of apparatus and the direction of the error for the different temperatures below the normal. For further explanation see Fig. 1.

cold objects, *e. g.*, a silver dollar, we may expect that the dermal stimulus will be felt as longer than under the ordinary conditions. This is exactly what occurs. Taking the curves as a whole, the unmistakable tendency resulting from the use of hot and cold stimuli is to reduce the amount of underestimation of

the cutaneous line, and with the extremer temperatures this underestimation gives way to a pronounced overestimation. But even with those temperatures which are most effective in changing the direction of the error, we observe, as in the pressure tests, certain points at which the estimation of the cutaneous stimulus seems to rest upon factors that are in large measure independent of the absolute pressure and the absolute temperature. Thus it will be noticed in Fig. 3 that the dermal line 2 cm. in length is persistently underestimated with the coldest temperatures, as well as with those up to and including the normal, although the lines on either side of it are with those more intense temperatures overestimated. It is, furthermore, just at this point that the hot stimuli begin to produce departures from the normal underestimation. The lines 7 and 7.5 show a similar tendency to revert to the primary underestimation, only the very hottest stimulus displaying resistance to the tendency. The subject upon whom these temperature tests were made shows a noticeable disposition to increased accuracy of judgment with the longer lines, both for the normal tests and for the temperature tests. The very short lines are also judged with considerable accuracy, while those of medium length show the largest errors.

Turning to Tables I. and II. it is interesting to observe that under the 'normal' conditions the percentage of correct judgments, overestimations and underestimations is for our two subjects almost identical. Each has 18% of overestimates, but the subject for the temperature tests has 66% of underestimates and the other subject 67%, the percentage of correct judgments being 15% and 16% respectively. This fact, together with the general similarity of the two normal curves, as seen, for example, in Figures 1 and 3, permits a more confident comparison of the general results of the two experiments.

Table III. shows that with the introduction of moderate heat we meet increasing irregularity of judgment, only 9% being correct, although the percentage of underestimates is slightly enlarged. As the heat is made more intense, the predominance of underestimates rapidly gives way to a large predominance of overestimates. In the case of the hottest stimulus this is accompanied by the same percentage of correct judgments as with the least intense heat stimulus.

TABLE III.

Average percentages for all lengths of apparatus.

	CORRECT JUDGMENTS.	UNDERESTIMA- TIONS.	OVERESTIMA- TIONS.
Normal Temperature.	16	66	18
Temperature 50° C.	9	16	75
Temperature 45° C.	15	31	54
Temperature 40° C.	9	69	22
Temperature 18° C.	19	37	44
Temperature 13° C.	20	40	40
Temperature 9° C.	13	44	43

Table III. also shows that with the particular temperatures employed the cold was less effective in producing large numbers of overestimates than the heat. When the amount of the overestimates is taken into account, however, as distinct from the mere number of such judgments, the disparity is very small. This will readily appear from comparing Figures 3 and 4. Moreover, the percentage of cases of overestimates and underestimates arising with the different degrees of cold is much more nearly constant than with the corresponding instances where heat is employed, nor is there the same amount of difference in the curves for the various degrees of cold as for those of heat. This would seem to be contrary to the observed effects of cold and heat upon the judgments of weight and superficial area. It may be that it has its explanation in fatigue of the end organ, although every care was exercised to prevent such a result. It may be, too, that the hottest stimulus was nearer the pain limit than the coldest one and that the disparity springs in part from such a source. This, however, was not felt to be the case, and it would in any event hardly serve to explain the facts as regards the two middle temperatures used.

Table II. shows typical cases of the reduction in the amount of the average error resulting from practice. In only one in-

stance was there a failure to reduce the amount of the error, which remained to the end an error of underestimation. The subject for the pressure tests, it will be remembered, gave overestimates at the outset for the short lines. This overestimation disappeared under practice and became an underestimation, while in general the average amount of original underestimations was very slightly altered, sometimes in one direction, sometimes in the other. The total effect of practice seems for both subjects, therefore, to be in the direction of fixing as permanent a small underestimation. It may be well to emphasize in this connection that neither subject was at any time during the experimentation informed of the correctness or incorrectness of the judgments made. Consequently, the practice effects represent as purely as possible the results of trained attention and discrimination.

We may say in *summary*, therefore, (1) that in comparing dermal linear extents under conditions of passive pressure with optical linear extents the temperature of the dermal stimulus is of distinct importance; (2) the normal underestimation of such stimuli is diminished by the introduction of temperature, and (3) with stimuli which are distinctly hot or cold the underestimation is changed to overestimation; (4) our experiments warrant no attempt to connect in detail the amount of such changes in the estimates with the amount of change in the stimulus.

The actual process by which the judgments were made varied somewhat with the two subjects, although the increasing ease, rapidity and immediacy of the act was, as time went on, very marked.

One subject began with the mediation of a motor image of measuring the cutaneous line with the joint of the thumb. This motor image was then identified with a visual line. The judgment was slow and difficult at the outset. At length the process became easier and the motor imagery dropped out altogether. As soon as the stimulus was given, the eye ran rapidly over the lines until one was found, which was immediately and unhesitatingly picked out as correct. None of the surrounding lines seemed to be at all satisfactory and there was seldom any

doubt. The arm was never visualized by this subject, nor did visual imagery seem to play any part in the process. The normal judgments were on the whole more accurate and constant than those of the other subject.

The second subject employed visualization to a great degree. When the cutaneous stimulus was applied, a visual image of the part touched at once arose and from this as a basis the judgment was formed. In both cases, then, it would seem that the errors met with involve, and in part rest upon, the intermediary imagery connecting the cutaneous linear experiences with those of vision. Naturally, as practice increases, the various cutaneous stimuli come through the ordinary process of association to serve as symbols of certain visual extents. When the connection becomes firmly established, the original intermediary imagery is likely to disappear from consciousness and the judgments become essentially immediate.

CONCERNING THE SIGNIFICANCE OF INTENSITY OF LIGHT IN VISUAL ESTIMATES OF DEPTH.

By M. L. ASHLEY.

The following experiments were made with a view to testing a hypothesis in regard to the influence of intensity of light in judgments concerning the third dimension.

It was believed that, while convergence, accommodation,¹

¹A very full bibliography on the subject of visual space perception is given in Helmholtz's 'Physiologische Optik.' Among the many recent experimental investigations of convergence and accommodation may be mentioned:

Bourdon, 'Expériences sur la perception visuelle de la profondeur.' *Revue Philosophique*, 43, p. 29.

Arrer, 'Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung.' *Philosophische Studien*, Bd. XIII., S. 116, 222.

Hillebrand, 'Das Verhältniss von Accommodation und Convergenz zur Tiefenlocalization.' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. VII., S. 97.

E. T. Dixon, 'On the Relation of Accommodation and Convergence to our Sense of Depth.' *Mind*, N. S., Vol. IV., p. 195.

W. Wundt, 'Zur Theorie der räumlichen Gesichtswahrnehmungen.' *Philosophische Studien*, Bd. XIV., S. 1.

The interest in these discussions, especially the first three, was, of course,

size of the so-called retinal image, aerial perspective,¹ disparity of retinal images, shadow, together with comparison with other objects, and similar secondary means of judging, had generally been acknowledged as factors influencing our estimates of the distances of objects, there still remained another factor, light,² or brightness, which was entitled to more recognition than it had yet received.

Convergence, accommodation and the other factors mentioned above as entering into judgments of depth become such factors or signs of depth, because they are impressions entering into or involved in the perception of distant objects and, under ordinary conditions, vary in some regular way as the distances of the objects vary.

These various factors do not have equal values under all circumstances as signs of distance. Some, such as convergence and accommodation, are almost always concerned in our estimates of depth, within certain limits, but lose their effectiveness for objects beyond those limits. Others, such as disparity of retinal images, aerial perspective, shadow, size of retinal image and comparison with other objects, often give us little or no aid.

Changes in distinctness of outline and in color also accompany changes in distance, but while they are usually connected

convergence and accommodation. Bourdon mentions, however (p. 41), that in some instances a lantern or bright object was judged nearer when brighter. Arrer also speaks of the influence of light, for he says (p. 245) that changing illumination seemed to have an influence in judging the distance of a thread which was used as object.

¹ When, in speaking of the influence of aerial perspective, Helmholtz says ('*Physiologische Optik*,' 2 Aufl., S. 774) that in the case of a snow-capped mountain glistening in the sunshine we are likely to underestimate its distance, he cites an instance in which there may be another cause concerned in producing the illusion, viz.: the increased amount of light reflected from the snow-topped mountain. In cases of aerial perspective there is also more or less change of color involved, but while distinctness of outline and shade of color depend in a measure on intensity of light, they are to be distinguished from it.

² In Hermann's '*Lehrbuch der Physiologie*,' p. 607, there is a statement in which direct reference is made to light itself. After mentioning other means of estimating distance the writer says: "Indirecte Entfernungsschätzungen finden ferner statt aus den relativen Verschiebungen der Gegenstände bei Bewegung des Kopfes, ferner aus der in der Entfernung abnehmenden Lichtstärke."

with variation in illumination, they are to be distinguished from it psychologically as means of judging the distance of objects. It is clear that the farther distant an object is the less light we receive from it, either directly, or indirectly by reflection; and, aside from surrounding objects, which may exclude some light from its surface or reflect more upon it, the intensity or amount of light reaching a given portion of the retina would vary inversely as the square of the distance of the object. It was believed that if monocular vision were used, in order to exclude as far as possible all advantage arising from the use of two eyes instead of one, and the retinal image kept constant, it would be found very difficult to estimate the distance of an object provided the amount of light received from it were kept constant; or, if retinal image and actual distance were constant, that the distance could be made to appear to vary by increasing or decreasing the intensity of the light.

At first monocular vision was tried with somewhat varied objects. After quite uniformly favorable results had been attained with one eye, both were tried, and it was found that convergence and whatever other advantages both eyes possessed failed to prevent marked cases of illusion, even within limits where convergence was supposed to be most effective.

The monocular experiments will be described first.

I.

MONOCULAR EXPERIMENTS.

In order to keep the retinal image constant while the distance varied, a tube was constructed through which the observer looked at an upright sheet of paper. At whatever distance the paper was placed, the observer saw a round object which seemed to remain constant in size. A Welsbach lamp was placed directly behind the paper. The observer, being ignorant of the location of the light, saw only a round luminous object. Different papers were used, some of which showed marked irregularities as the light became stronger, but became more homogeneous as the light became fainter. One of the objects was a blue gelatine paper covered on

the back with a yellowish colored paper. This gave an almost perfectly homogeneous surface. Another paper was covered with fine dots and marks, which came into view when the light became strong and faded away as the light diminished.

The Welsbach lamp was altered so that the gas supply could be better regulated, and was furnished with an index which showed the intensity of the light in units of one candle-power. Fig. 1 shows the arrangement of the apparatus.

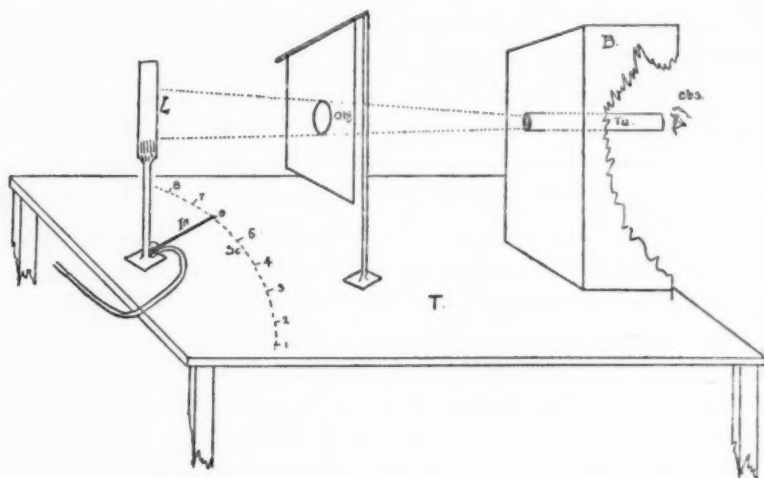


FIG. 1.

Obs represents the position of the eye of the observer, who looks through the tube, *Tu*, at the object, *Obj*, suspended between him and the lamp, *L*. *In* is an index and *Sc* a scale which measures the intensity of the light. The tube, *Tu*, has a large chamber between its two ends which prevents glimmering from its sides. The box, *B*, into which the tube, *Tu*, is fitted is blackened inside. A focussing-cloth covers the observer's head to exclude light as far as possible. The apparatus is arranged upon the table *T*.

Subjects A, B and C.

A, B and C looked at the same object in the same position, viz : a coarse, white paper suspended between the observer and the lamp, twenty inches from the lamp and six inches from the sub-

ject. The light was gradually increased, while the observer watched the luminous disk. The judgments were given in sets of ten each, starting with the lamp at one, three and seven candle-power respectively. Subject A gave seven sittings to the experiment, thus giving two hundred and ten judgments in all. With increased intensity of light he judged the object nearer in every instance but one, in which case he thought the distance unchanged.

It was found, upon taking an average of the ten judgments given each day from each of the three points of reference, that if one candle-power was made a starting-point the object was judged nearer, when 3.05, 2.05, 2, 2.15, 2, 2.45 and 1.84 candle-power was reached on successive days respectively. When three candle-power was taken as a point of reference, the ratio of increase required for similar results was 2.7, 2.03, 2.13, 2.15, 2.23, 2.11, and 1.98 on successive days; and when ten candle-power was taken to begin with, the ratio of increase reached 2.42, 1.9, 1.73, 1.8, 1.77, 1.71, and 1.74 on successive days. Upon being asked to report changes in brightness, he announced these when the light had increased about half as much as was required to make the object seem nearer.

On the first day B judged the object nearer in each case but one. The next day a few were judged 'the same' when three candle-power was taken as a point of reference, and nearly all were judged 'the same' when the start was made from seven candle-power. On the last day the object was judged to be the same distance as the point of reference in nearly every instance. In instances where he judged the object nearer, the change in candle-power was approximately the same as with A. On the first day B said that the object seemed to expand as it came up. On the second day he gave his judgments more deliberately and remarked that if he waited a short time it seemed to approach instead of merely growing brighter. The following days he said he tried to see whether it really did move or not, but could not see any actual motion.

Like subject B, C judged nearly every case 'nearer' the first day, fewer 'nearer' the second day, and nearly all 'the same' on the last day.

He remarked that there was often a conflict between his feeling and his judgment. He felt at first as if the object approached and there was a strong impulse to say 'nearer,' but after waiting the change seemed to be only in brightness. He said that if he did not reflect, he would judge it coming nearer in every instance. His judgments depended, he said, on whether he could make himself believe it was all due to light.

Subject D.

Subject D looked at a paper covered with fine dots and lines which came into view under increasing light and thus gave the object even more appearance of approaching.

The paper was arranged as in the preceding experiments, with the exception of being fifteen inches from the lamp and eleven inches from the observer. The first two or three days paper without the dots and lines was used. D did not judge readily that this approached. On the following days, especially when the dotted paper was used, he judged it nearer almost every time, the amount of required change in light being approximately the same as with the white paper used with former subjects. A number of times the light was diminished instead of increased, and then he judged the object to be receding, after the light had decreased in about the same ratio that it had to increase in order to bring about a judgment of 'nearer.'

Subject E.

After trying different papers, it was found that E gave the most uniform results when the homogeneous gelatine paper, already referred to, was used. The gelatine paper was placed eight inches from the lamp and about eighteen inches from the subject. Both sudden and gradual changes were tried. When the change was sudden, he was given a view of the object as it appeared at a certain degree of illumination; then the tube was suddenly closed by a lid, while the light was increased or diminished. He then looked again and judged what change, if any, there was in the distance of the object. The time consumed in making the change was three or four seconds.

It was found after three or four days' trial—the customary three sets of ten judgments each having been taken every day—that beginning with one candle-power the change in illumination must reach about two candle-power before he could be sure the object was nearer. If the start was made from two candle-power, the change must be to about four and one-half. If three candle-power was taken as a point of reference, then the change must reach about seven candle-power in order to be judged nearer. On the last day gradual changes, similar to those employed with the other subjects, were used, with no marked change in the results. In two or three cases E judged that the object receded. He said that sometimes the object seemed covered at first with a film; then the film disappeared and the object approached. When the light was diminished instead of increased, he thought the object receded. An occasional experiment of this latter sort was made to determine whether the subject would be consistent, when the direction of the change in light was reversed.

GENERAL RESULTS OF MONOCULAR EXPERIMENTS.

Besides the subjects whose judgments are referred to above, two or three others were tried whose judgments were too irregular to be of value in determining any very definite relation between variation in intensity of light and apparent change in distance. For the most part, however, they agreed with the others in judging the object nearer when brighter and farther when dimmer.

Besides these last-mentioned persons, ten or a dozen others gave one sitting to the experiment, the purpose being to see how they would be impressed, rather than to obtain any numerical results. They were shown various different colored papers at various degrees of illumination. Each one tended strongly to judge the object nearer when brighter and farther when its brightness diminished. Occasionally an opposite judgment would be given and occasionally the distance would be thought the same, but on the whole the results were quite regular.

Sometimes the paper would be moved toward the lamp and away from the observer, who would then think it approaching.

If moved toward him and away from the lamp, so as to decrease in brightness, he would say it was going away.

The observers did not know the arrangement of the apparatus and did not know they were judging wrongly. They were not told that the paper moved when it was kept stationary, but were asked whether it moved or not and in which direction. Sometimes when the paper was actually moved, they were told that it moved, but then were deceived in respect to the direction of the movement—thinking it moved away when it came nearer, and *vice versa*. From many remarks made by observers it was clear that the change in brightness was noted, in general, much before a distance change was felt.

When the distances of paper and lamp from the subject are constant, accommodation should remain constant. In monocular vision disparateness of retinal images would be avoided. Convergence would also be avoided as far as possible, and consequently judgments of 'nearer' and 'farther' could not well be due to anything else besides changing illumination, with whatever change there might be in distinctness, color and seeming change in size. In the case of the white papers, especially, the change in color would be little more than change in brightness, and with the white papers and the gelatine the outline of the circular object could be clearly seen, except at quite faint illumination. Whatever small irregularities came into view on the surface under stronger illumination would tend to give the impression of an object coming sufficiently near for its surface to be more clearly seen. In the case of the gelatine paper the main change was in color, from dark to a light purple, the surface being almost perfectly homogeneous.

The change in size, which was noticed by certain subjects, might be due in part to stimulation of a larger tract of the retina by the stronger light; in part it could be attributed to illusion caused by thinking the object approaching. Some of the observers said the object appeared to approach and then stop. This might be due to the fact that changes in accommodation and retinal image did not follow the supposed approach. The seeming check in approach may be explained, also, as due to a break in the act of accommodation. It is very

likely that in all judgments of distance under changing illumination such associated changes in accommodation may participate as factors. The iris is accustomed to contract as accommodation for near objects takes place, and, as it contracts with increasing illumination, accommodation may follow. Supposing this to occur, this element would probably be most effective when changes were gradual instead of successive, and would be significant only within narrow limits, for the blurring of the object, which results from improper accommodation, would check the accommodation movement.

The film mentioned by one subject was, perhaps, due to improper accommodation at the starting-point, or to the object growing more distinct under increasing light.

It was found that, starting at as low a degree of light as would enable the paper to be seen fairly clearly, the ratio of increase must be about two—*i. e.*, the intensity must increase to about twice that of the point of reference—before the object would be judged nearer. If the light was decreased there would be a judgment of 'farther' given, when it had reached about half its former intensity.

II.

BINOCULAR EXPERIMENTS.

The binocular experiments may be classed under three heads, the third having four varieties.

1. In the first binocular observations the arrangement of apparatus was the same as shown in Fig. 1 with the exception of the tube. Instead of a tube, the observer looked through horizontal slits on opposite sides of the box, the slits corresponding to the ends of the tube. The slits were large enough to avoid getting double images within the distances used. The subject looked at a bright surface with both eyes instead of one, as in preceding experiments. As in the monocular experiments, various sorts of papers were used—white, colored, speckled and homogeneous gelatine paper.

Subjects A, B, C, D and E each gave a trial to the binocular experiments. The results with subjects B, C, D and E were not

materially different from those obtained from them in monocular vision. A's results, of which alone brief mention will be made, were very regular and corresponded closely to those he gave before in monocular vision. The discrimination was even finer than in monocular vision, the averages showing a ratio of change in intensity of light of 1.67, 1.63, 1.37 as the starting-point was taken at one, three or seven candle-power respectively. The same sort of paper was used as in A's monocular observations, the only difference being that the paper was seven inches farther from the lamp and seven inches nearer the subject than before. The paper was, of course, stationary. A was quite certain it approached in each instance, and remarked, "It comes right up to a fellow!"

Subject G.

Another subject, G, also gave very regular results. The paper used was covered with fine dots and was placed fifteen inches from the lamp and eleven inches from the observer. At the first sitting one judgment was given in which the object was thought farther. In all other instances he judged the object nearer with increasing light and farther when the light decreased, the average ratio of change in intensity of light being about 2 in either case.

Again, with subject G actual movement of paper was tried with constant light. He judged slight movements correctly. Then, when movement toward him was combined with decrease of light, he judged the object farther. When the paper was moved away from him with increasing light, he judged it approaching.

2. In the next form of binocular experiment the lamp was placed at the side of the box instead of in front of it. As object an upright stick, about the size of a lead-pencil, was placed in the line of vision of the two slits in the box. The arrangement is shown by means of Fig. 2.

Fig. 2 represents a box in which are two slits, through which the observer, *Obs*, looked at the object, *Obj*, from which light is reflected from the lamp, *L*. *Bg* is a black background, about four feet beyond the object.

The object was moved back and forth on the table from twenty to forty inches from the observer. As in the other experiments, the subject's head was covered with a focussing cloth.

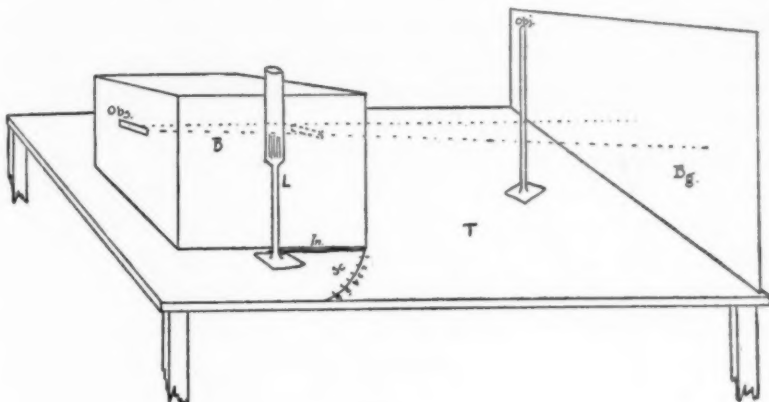


FIG. 2.

Three or four subjects looked at the object arranged in this way. One was rather uncertain when the intensity of the light was changed, sometimes thinking the object farther, sometimes nearer. Fairly definite results were obtained from two others. They judged correctly as to actual movement, but were deceived when the light was either diminished or increased. If the light was constant they could judge correctly after a movement of one and one-half inches.¹ If the light was increased in a marked degree while the object receded, they would judge it nearer; if, on the other hand, the object was moved nearer while the light was diminished, they judged it farther away. The actual movement in these cases of illusion was approximately the same as was required for correct judgments when the light was constant.

An objectionable feature in this form of experiment was that the background as well as the object grew light or dark, as the case might be. It would be unnatural for the whole field of view to become lighter when some single object approached.

¹ Wundt has found that at short distances a change $\frac{1}{3}$ of the actual distance is sufficient to enable one to discriminate a change in distance. The greater change required here is probably due largely to lower degree of illumination.

Accordingly, plans were devised to keep the background at a fairly constant degree of illumination while the object itself changed in brightness. The demand for a uniformly illuminated background led to the third form of binocular experiment, in which two sources of light were used—one to give to the background a fairly constant illumination, and the other to throw light of varying intensity on the object. Four varieties of this third form were tried, differing in the nature of the background. The varieties are classed as a, b, c and d.

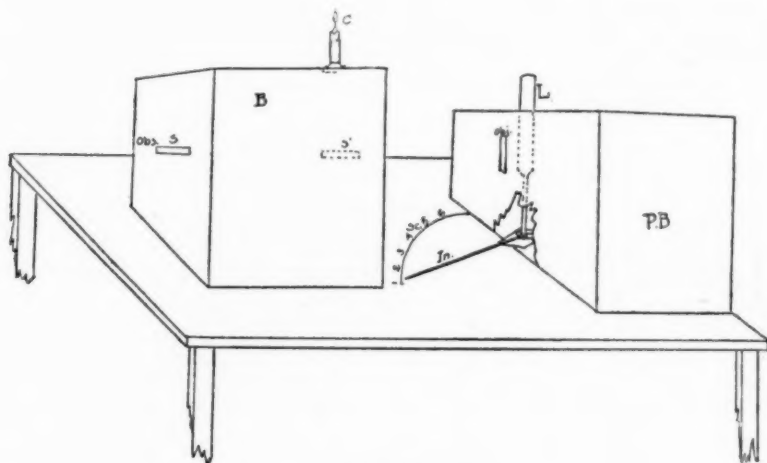


FIG. 3.

3 a. In the first variety of the third form the apparatus was arranged as shown in Fig. 3. *B* represents a box with slits, *S* and *S'*, as in the preceding diagram. *PB* is a pasteboard box enclosing the lamp, *L*, and serving as a background. Within this field is the object, *Obj.* The object is made by cutting an upright slit in the pasteboard box and covering it with transparent paper, in such a manner that the paper bulges outward over half an inch and, with little or no illumination from within, looks to the observer like an upright stick an inch in diameter, standing directly in front of the background, which is painted black. The upright object has a yellowish-white appearance. A candle, *C*, gives a constant illumination to

the background. *Sc* is a scale by which the intensity of the lamp can be graded. The object in these experiments was forty-four inches from the observer.

When the observer first sees the upright object, it is illuminated, together with the background, by the candle *C*. Then, when the light within the pasteboard box is increased, the object becomes lighter and seems to approach and separate from the background, which appears to recede and becomes indefinite as to position, or else attention is fixed on the object and the background forgotten.

This arrangement of apparatus was found very effective in producing illusions of movement. Four persons, H, I, J and K, each gave a number of sittings to this form of experiment. In nearly every instance each person judged the object nearer when its luminosity increased and farther when it decreased. Starting-points of .25, .5, .75 and 1 candle-power were taken and the ratio of change required to give an illusion of approach found to vary from 1.5 to 3. When the lower intensities were taken as starting-points, the required ratio of increase was less. The results from the four subjects did not differ materially.

In general, the subjects distinguished between changes of brightness and what appeared like a change in distance. If asked to notice when the object grew brighter and when it seemed to approach, they generally reported the alteration in brightness when the change had reached about half that required for the impression of 'nearer,' which they gave a little afterwards. Besides these subjects, four others gave one sitting to the experiment and were similarly impressed.

3 b. In the next variety of experiment, in which two sources of light were used, the lamp giving the variable light was placed below and in front of the box, while the candle, now enclosed in a paper shade, remained on top of the box. The lamp was enclosed in a pasteboard box in which was a small opening, which allowed a beam of light to fall on the object, but at such an angle that it did not strike the background within the field of view. By this means the background illuminated by the candle would remain practically constant in brightness, while the object could be made brighter at will. The background

was too far away and too dim for any shadow from the object to be seen.

Fig. 4 shows the construction of the apparatus. *B* is a box with slits *S* and *S'*, through which the observer *Obs* looks and includes in his field of vision the background within in *V—V'*. The background is formed by a dead-black curtain. The background is farther from the observer and *V—V'* larger than would appear from the diagram, but the portion of the background *O* illuminated by the beam of light from the lamp *L*

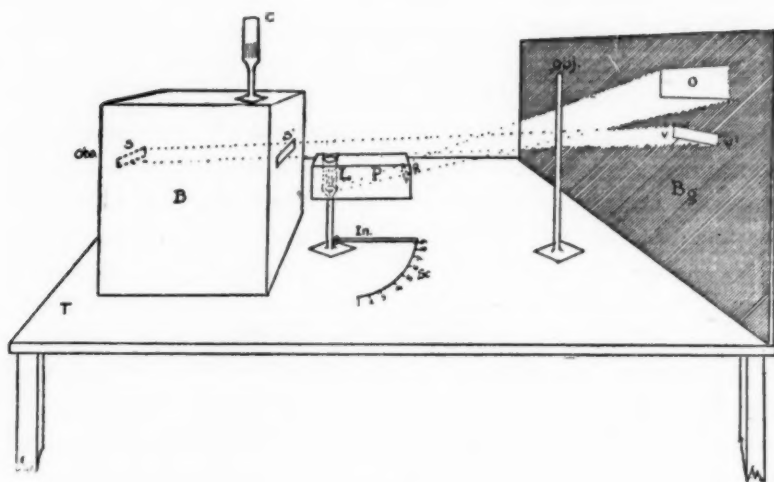


FIG. 4.

falls above *V—V'*. *P* is a small pasteboard box enclosing the lamp *L*, provided with a slit *R*, through which a beam of light passes, striking the object *Obj* in the observer's line of vision. *Sc* is a scale to regulate intensity of light by means of index *In*. *C* is a candle with shade, and *T* the table on which the apparatus is arranged.

With this apparatus various results were obtained. Out of eight persons who gave a few sittings to this variety of experiment, two seemed to be able to distinguish light changes from distance changes and were not much inclined to think the distance varied with change of illumination. One was rather irregular, judging sometimes one way, sometimes another, but

was inclined to judge the object nearer when bright. Another subject L distinguished to a certain extent between brightness and distance, in that brightness was clearly noted first, and then as it kept increasing the object was felt to come nearer. Sometimes, however, there was little or no impression of a change in distance.

The light of the candle giving constant illumination to the background was low, the shade decreasing it to not more than a tenth of one candle-power. The upright used in this variety of experiment was a round stick one-half inch in diameter, covered with whitish paper on which were fine dots and lines. The object was forty inches from the observer.

When the lamp was gradually increased from zero candle-power, subject L noted a change in brightness of the object at .5 candle-power, and, when 2 candle-power was reached, thought it approached. If .5 candle-power was taken as a starting-point, brightness and approach were noted at 2 and 5 candle-power respectively.

Subject M.

M was rather irregular. When the object was stationary and only the light changed, there was no marked tendency to judge the object nearer or farther. When the lamp was turned so low as to be negligible, an increase of six inches in distance could be detected fairly well starting from forty inches, the changes being made while the view of the object was shut off by covering slit *S'* with a piece of cardboard.

A combination of changes was now tried during which the following judgments were given.¹ Column L marks the increase in candle-power starting at zero. Column D marks the distance in inches that the object was moved from the point of reference and away from the subject. At the reference point the object was forty inches away and the lamp at zero candle-power in each case. In column J, giving judgments, s means 'the same,' and f indicates 'farther.' M was given a view of the object as a point of reference. Then, after the slit *S'* was closed,

¹The following experiments were of such a nature that the results can be shown best in tabulated form.

changes were made in the light and in the distance of the object. *S'* was now opened and the subject asked to judge whether the object was the same, nearer or farther.

M's RESULTS.

L	D	J	L	D	J
2	4	s	2	8	s
2	6	s	2	9	f
2	7	s	2	8	f
2	8	f	2	8	s
2	8	f	2	9	s
2	6	f	2	9	f
2	5	s	2	10	Doubtful
2	6	s	2	11	f

The increase of light seems here to have had little more than a disturbing influence. At least, there is no very marked tendency to let increased brightness compensate for an actual change in distance.

Subject N.

In the case of subject N the influence of light, both alone and in combination with actual movement of the object, was much more pronounced.

When the lamp was turned down, N could detect an actual movement nearer of three inches, and a movement away of five inches. The reference point was forty inches away in all instances. When the object was stationary and forty inches distant he generally judged it nearer when the lamp reached two and one-half candle-power; or, starting from this point, he would judge the object farther when the light from the lamp was decreased to zero.

The table below shows the results of combining change of light with change of distance. As with subject M, in every instance a point of reference was given and then the adjustments were made while slit *S'* was closed.

Column L shows increase of lamp-light in candle-power; D, increase of distance in inches; J, judgments of same, nearer and farther.

Other judgments were taken in which the object was moved nearer while the light was decreased, starting from two candle-

power. In these instances an actual movement of four or five inches nearer would be often judged 'the same.' Also actual movements away of ten inches were judged 'the same' when the lamp meanwhile increased from zero to eight candle-power.

N's RESULTS.

L	D	J	L	D	J	L	D	J	L	D	J
2	4	s	2	9	f	2	4	s	4	5	n
2	6	s	2	9	f	.75	5	f	3	5	s
2	8	n	2	8	f	1	5	s	3	5	f
2	10	f	2	8	s	2	5	n	5+	5	n
2	9	f	2	7	s	3	5	f	5	5	n
2	7	s	2	6	f	3	5	n	5	5	n
2	8	s	2	6	s	3	5	s	5	5	n
2	8	f	2	5	s	4	5	n	5	5	n
2	9	f									

It will be clear from the tables that in one set of judgments the increase in light was the same each time, while the distance increase varied with an accompanying variation in judgment. In the other set distance change was constant, while the light increased. In the first set an increase of two candle-power seemed to compensate for an increase in distance of about three inches. In the second set five inches increase in distance seemed to be more than offset by an increase of five candle-power in lamp-light.

Subject O.

Subject O judged on the whole much as N had done. Conditions were same as with N, viz.: reference point at forty inches, constant light from shaded candle, variable light from the lamp and same object.

With the lamp at zero an increase of three inches or decrease of two inches in distance would generally be correctly judged. If the distance remained constant, an increase of lamp-light to two candle-power would be called 'brighter and nearer.' A change from two candle-power to zero would be judged farther. Combinations were now tried, some results of which are given in the table below. In the first set recorded here the background was the one that had been used throughout this form of binocular experiment. In the second set a whitish background was sub-

stituted for it and placed nearer, being located forty inches beyond the object.

O's RESULTS.

FIRST SET.						SECOND SET.					
L	D	J	L	D	J	L	D	J	L	D	J
2-0	1n	d & f	0-2	0	b & n	2-0	3n	f	0-2	3f	n
2-0	2n	d & f	0-2	2f	b & n	2-0	5n	f	0-2	4f	n
2-0	3n	d & f	0-2	3f	b & s	2-0	7n	f	0-2	5f	n
2-0	4n	d & s	0-2	4f	b & s	2-0	8n	f	0-2	6f	n
2-0	5n	d & s	0-2	5f	b & f	2-0	9n	f	0-2	7f	n
2-0	6n	d & n	0-2	6f	b & f	2-0	9.5n	s	0-2	7f	n
2-0	7.5n	d & n	0-2	7f	b & f	2-0	10n	s	0-2	8f	n
2-0	8n	d & n	0-2	8f	b & f	2-0	12n	n	0-2	9f	n
2-0	9n	d & n				2-0	10n	n	0-2	10f	s
						2-0	12n	n	0-2	11f	s
									0-2	12f	s
									0-2	14f	f
									0-2	18f	f
									0-2	20f	f

In columns marked L changes in the lamp-light from zero to two, or from two to zero, are recorded. Columns marked D show the increase or decrease of distance in inches that were combined with these changes in the light; n stands for nearer and f for farther. In columns marked J, n, f, b and d mean that the object was judged nearer, farther, brighter or dimmer, as the case might be.

In the first set it appears that with light-change from two to zero an approach of six inches was required before the object was judged nearer. In the second set double this actual movement was required for a similar judgment. In the first set a light-change from zero to two had to be combined with five inches' actual movement away to give an impression of farther, while in the second set from twelve to fourteen inches was necessary. In each set, when the turning-point was reached, where there was a transition from judgments of 'nearer' to those of 'farther' or *vice versa*, there was much perplexity on the part of the subject, who seemed to think there must be something wrong subjectively. It appeared that conflicting factors were felt, and, until one became strong enough clearly to outweigh the other, there was considerable difficulty in telling what had taken place in respect to movement. A similar thing may

also be said of subject N in the case of similar combinations. The white and nearer background seemed to be a disturbing rather than helpful factor.

3 c. In this variety of binocular experiment the apparatus was the same as described under 3 b, except that two objects were used, the farther of which remained the same as to light and distance, while the first varied in brightness. The first and variable object was the same one that had been used up to this point, and was located thirty-one inches from the subject. The other object was placed a little to one side of this first one and twenty inches farther away. It was expected to serve as background and supplementary means of judging whether the first moved or not.

The subject was given a view of them both at once and then, after the brightness of the first had been altered, was asked whether they were the same distance apart. When the lamp, which illuminated the first, was increased from zero to eight or ten candle-power, the objects were thought to be separated farther and the first or bright one generally seemed nearer the subject. A decrease of light from eight or ten candle-power to zero would give the impression that the objects had come closer together. Subject O gave judgments of this nature, and also another person who did not readily get impressions of distance change in the variety of experiment described under 3 b.

3 d. In this variety of experiment the apparatus was very similar to that used in 3 a. The modifications were a decrease in intensity of the candle used for constant light to about one-tenth candle-power, and a second object placed between the observers and the upright luminous object used in 3 a. This second object was the upright one used in 3 b. It was placed about two feet from the subject, so that it appeared to be much nearer the subject and a little to one side of the other luminous variable object, which was about fifty-six inches away. This arrangement showed two objects, one beyond the other and far enough to one side not to be confused with it. The subjects looked at the two objects while the brightness of the farther one increased or diminished.

Subject O gave a few judgments in respect to the distances

of these objects. When one, *i. e.*, the nearer one, remained constant in brightness and distance, and the farther one varied in brightness, the farther one was often judged to approach. The results, however, were rather conflicting, the farther one sometimes seeming to approach or recede as it became brighter or dimmer, while the first one seemed stationary. At other times the first one seemed to check the tendency to judge the other approaching or receding.

Two other persons who made a few observations had similar impressions of distance change when the brightness of the farther object varied. This was especially the case when the objects were even farther apart. When nearer together, there did not seem to be so much impression of change in the distance of either.

GENERAL RESULTS.

The various sorts of binocular experiments were devised to give varied and consequently greater evidence as to the influence of intensity of light as a factor in estimates of depth. It has been found, as a rule, of marked importance, even where accommodation, convergence, size of retinal image and disparateness of retinal images could enter to oppose it; and even within the short distances tried, where the other opposing factors would be thought most effective, it has been shown to be equivalent in some instances to a certain amount of actual distance change.

In the monocular experiments something very much like Weber's law was found in the relation of increase of light to apparent decrease in distance of the object. The distances used as points of reference have been small, but there is reason to suppose that at larger distances where accommodation, convergence and disparateness become of little or no value, the relative importance of light as a factor in distance judgments would increase.

As mentioned before, the subjects did not understand the arrangement of the apparatus, and throughout the experiments care was taken to avoid as far as possible the matter of suggestion. In both monocular and binocular experiments, sub-

jects have mentioned changes in distance without expecting to see distance changes and without having been asked whether they saw such changes. This is especially true of subject O, who gave very definite results in the combination under 3 b.

While there is room for far more extended investigation in respect to both larger distances and more definite numerical determinations, it appears that the hypothesis made at the beginning has received considerable evidence in its support.

In conclusion, I wish to acknowledge my indebtedness to Professor J. R. Angell for many helpful suggestions in respect to different phases of the investigation. My thanks are also due those who very kindly served as subjects in a work which for them was less interesting, because its real object was not understood.

A STATISTICAL STUDY OF BELIEF.¹

BY FRANCIS BERTODY SUMNER.

I. INTRODUCTION.

It is a matter of common experience that our beliefs are more or less graded in a scale of certainty. Some of them we cannot conceive of our ever changing; others, although practically certain, might, conceivably at least, be subject to revision or even reversal, whilst others yet command little authority and even vary momentarily with the play of our feelings. Nor are these three main ranks of certainty all that can be distinguished. The degree of one's belief, like the intensity of any mental state, admits of an almost indefinite gradation quantitatively. But, unfortunately for our present purpose, it shares with other mental states the property of defying accurate measurement. The practical test of belief is, of course, conduct. One can be truly said to believe only what one acts as if he believes, and the gauge of his conviction is the extent to which it is embodied in his actions. Thus, although beliefs defy exact measurement, and are even to a great extent incommensurate, one with the other, their relative strength finds a common standard in their motor results. But even this does not furnish a practicable yardstick to the psychologist. All those things of which we are practically 'sure' are in our daily life absolutely coercive upon conduct. There would be no degrees distinguishable. And this, although to introspection there might be appreciable differences in the intensity or vividness with which they presented themselves to consciousness. Hence, in the present experiments, I have had recourse to the method of introspection, although this is fraught with difficulties so great as perhaps to render the results of little value.

¹ From the Psychological Laboratory of Columbia University.

II. METHOD.

For my general method of procedure, I am indebted in large degree to the suggestions of Professor Cattell. This method is as follows: I drew up a set of twenty-five questions, 'a sort of shorter catechism of twenty-five articles of faith,' as a friend of mine called it. These questions bore upon a great variety of topics, being intended to touch nearly every general class of subjects upon which the average person forms opinions. Quite as diverse were the degrees of certainty with which the questions might be answered, some being answerable with absolute confidence, some with very little or none.¹ For convenience in performing the experiment, the questions, typewritten, were pasted upon separate cards. The subject was requested to spread the cards out upon a table and arrange the questions in the order of the certainty with which he felt able to answer them, placing the most certain at the top, the least certain at the bottom and the others in a series between. This arrangement was to take no account of whether positive or negative answers were given; it simply was intended to show the certainty with which the questions were answered, *i. e.*, the degree of one's conviction concerning the subject-matter of each. If two or more questions could, in the subject's opinion, only be answered with exactly the same degree of confidence, they were to be placed upon the same horizontal line. The following illustrates the plan:

D yes, B yes, I yes.
 V yes.
 N yes, F yes.
 R yes.
 O yes.
 A yes, L yes.
 U no.
 K yes, etc.

TABLE I.—THE QUESTIONS EMPLOYED IN THE EXPERIMENT.

- A. Is the world becoming better?
- B. Are there other human minds besides your own?
- C. Would this continent have become as quickly civilized if it had remained colonial?
- D. Do two plus two equal four?

¹For questions, see Table I.

- E. Will the death penalty for murder always be held justifiable among civilized peoples?
- F. Is the earth round?
- G. Will our Republic endure another hundred years?
- H. Does the present life alone furnish sufficient motives for moral conduct?
- I. Will the sun rise to-morrow?
- J. Is a protective tariff a wise policy for the United States?
- K. Do any landscape paintings yield as much satisfaction as the finest natural scenes?
- L. Is the evolution of living beings a fact?
- M. Is there life on other heavenly bodies?
- N. Did George Washington live?
- O. Will the most honest man you know be honest ten years hence?
- P. Is a man's conduct determined entirely by his heredity and the circumstances of his life?
- Q. Is the scientific mind as truly creative as the artistic?
- R. Does the moon's attraction cause the tides?
- S. Is there an even number of persons in New York City?
- T. Will there be frost in New York City during September next?
- U. Is matter ever created or destroyed?
- V. Am I awake at this moment, *i. e.*, not merely dreaming?
- W. Do spirits of the departed ever communicate with living persons? (We refer only to modern times.)
- X. Will poetry always be held in high regard by the most cultivated minds?
- Y. Would a college education be, on the whole, an advantage to the majority of young men?

III. DIFFICULTIES.

As I had expected, the experiment was attended with great difficulties. No list of questions of this sort can be constructed which is free from ambiguities. A paragraph of explanation for each one would be impossible practically, but would be necessary to an adequate understanding of the intended meaning.

A second difficulty is to be found in the impossibility of altogether distinguishing one's *subjective* feeling of sureness from the *logical* certainty of the thing believed in. The two are by no means always proportional, and of course it is the former that we wish to record. In one's despair at not being able to lay hold of the elusive belief itself, one is strongly tempted to go back to the data upon which it is based and to deal with them rationally. But if there is one thing which the student of this subject is impressed with it is with how small a fraction of our beliefs arise in the first instance through reason or, having arisen, are maintained by it. One believes most vividly what

appeals to one's interest most strongly, although to cold reflection the thing believed in may take a low place in the scale of certainty.

A third difficulty in the way of our study we have already alluded to, *i. e.*, the incommensurability of beliefs concerning widely different classes of things. Many of my subjects have complained with justice that their beliefs upon the matters in hand varied not only in quantity but in quality. As we have seen, there is a common standard of measurement in action, but such a test is here impracticable. So that the above difficulty is a serious one. I have, myself, tried with some measure of success the expedient of imagining my degree of surprise upon learning that the beliefs in question were false.

Yet another obstacle we have to deal with is the fluctuating and temporary nature of any arrangement we may make with the questions. A given belief rises or falls in the scale of certainty according to the momentary state of our mind's contents. The chance intrusion of a new idea may lead us to make a wholesale rearrangement of our order. Merely fixing one's attention upon any member of the series tends to raise its relative position. This vacillation diminishes, however, with further reflection upon the task, and one may succeed at last in giving what he feels is a fair record of his mean attitude towards the set as a whole.

IV. RESULTS.

1. *General.*—In all I obtained about one hundred and thirty records of this experiment. Of these, about thirty were defective, owing to carelessness or to misunderstanding of my purpose. I selected for study the hundred records which seemed to be most carefully made out.

In my work of securing subjects, I was aided very materially by several friends, whose labors secured me many records. My acknowledgments are especially due to Messrs. Paulmier, Holmes and Strong, of the Department of Zoölogy at Columbia; to Professor C. J. Herrick, of Denison University, and to Miss M. W. Calkins, of Wellesley. I wish also to record here my profound thanks to Professors Russell and Prettyman, of the Teachers College, New York, and to Professor Maria L.

Sanford, of the University of Minnesota, for allowing me to perform my second test¹ upon scholars and students in their respective institutions.

The average time devoted to the [first] experiment by each subject was about three-quarters of an hour.

Of the hundred whose records were studied, forty-two stated that their arrangement of the questions was satisfactory to themselves, twenty-four that it was fairly satisfactory, while twenty-four stated that it was unsatisfactory.

In a large proportion of cases the letters² were all arranged serially, in many others, on the contrary, certain ones were grouped together and given coördinate value. The lowest number of degrees of certainty discriminated was four, the average was a little over nineteen.

In the computation of averages, my procedure was as follows: The position assigned to every letter by each of the subjects was tabulated and the average taken. For example, if question A was given twelfth place in one subject's column, sixteenth place in another and eleventh in a third, the average position of A, for the three records, would be 13.

In cases where several letters were given coördinate value, the matter was a little complicated. A letter having tenth place in a series of 13 grades would clearly have a different value from one having tenth place in a series of 25 grades. Some method was necessary by which two such discrepant series could be reduced to common terms. Take the case of one having 13 grades or degrees of certainty. The first and last of these I denoted 1 and 25 as in the 25-grade series. But in the former case there are 12 intervals, in the latter 24. Accordingly I doubled each interval. 1, 2, 3 . . . etc., to 13 became 1, 3, 5 . . . etc., to 25. Thus it was possible to compute the average position of each letter for all the records.

Table II. gives the average position of each letter for the hundred subjects. D takes first place. If it had been given first place unanimously by all the subjects, its position in the

¹ See p. 629.

² In the discussion which follows, each question will be designated simply by its letter. See Table I.

average series would be 1. But by many it was given second or even a lower place. S by unanimous verdict¹ takes last place and its position is accordingly 25. Many subjects considered other questions—*e. g.*, T and M—to be as unanswerable as S and made them coördinate with it, but those letters were given a higher place in most records and therefore their average position is higher.

TABLE II.—AVERAGE ARRANGEMENTS OF LETTERS.

ALL SUBJECTS, 100.	WOMEN, 35.	MEN—NOT PSY- CHOLOGISTS, 37.	PSYCHOLOGISTS, 29.
D 1.7	D 2.1	D 1.7	D 1.5
B 3.9	F 3.8	N 3.5	V 2.3
N 4.1	N 3.8	B 4.1	B 2.8
V 4.2	B 4.7	V 4.9	F 4.7
F 4.7	V 4.9	F 5.6	I 4.9
I 6.0	I 6.5	I 6.5	N 5.2
H 9.1	H 7.3	R 8.7	R 8.4
R 9.4	U 9.8	H 10.6	L 8.5
U 10.2	L 10.9	L 11.0	U 9.2
L 10.2	A 11.0	U 11.2	H 9.3
X 12.1	O 11.2	A 12.3	P 11.1
A 12.1	X 11.3	K 12.6	Q 12.1
P 12.1	R 11.4	X 12.7	X 12.3
O 13.1	P 12.2	P 13.0	O 13.0
Q 13.6	K 12.4	Y 13.4	A 13.2
K 13.6	W 13.2	C 14.4	Y 14.8
Y 14.0	Q 13.2	O 14.8	J 15.5
W 14.9	Y 14.0	W 14.9	K 16.3
C 15.5	C 14.7	Q 15.0	G 17.0
J 15.6	E 15.2	J 15.3	W 17.4
E 16.4	J 15.7	E 15.7	C 17.8
G 16.6	G 16.5	G 16.4	E 18.8
M 19.8	M 18.9	M 20.2	M 20.3
T 21.8	T 21.9	T 21.8	T 21.6
S 25.0	S 25.0	S 25.0	S 25.0

Table II. (first column) exhibits, then, the average arrangement of the twenty-five questions by a hundred educated persons, most of whom, I am convinced, performed the experiment with care. Obvious at a glance is the fact that the intervals are not equal. D (1.7) is followed by B (3.9), but H (9.1) is followed by R (9.4). The relative spacing of the members of the series is best shown graphically (Chart I., *a*). It will be seen that the members of the series are arranged in three main groups.

¹Some, whose records were rejected, gave it a rank several places from the end.

great. The intervals within this group, however, are some of them very small, while three letters (X, A and P) occupy the same place in the scale as do also U and L, Q and K. For further understanding of the chart the reader is referred to Table I., where the meanings of the letters are given.

2. *Variations.*—An average is of little value, however, without a knowledge of the extent of variation among the numbers averaged. Table III. gives the average variation for each letter. Thus that of A is for the hundred subjects 4.7. This is the average departure of the A's in all the hundred records from the average A (=12.1). The average variation for all the letters is 4. This certainly shows a high degree of divergence among the records of the various subjects. If the average variation of a given letter (M, for example) is 4, this signifies that only a half of the M's of our subjects would fall within an interval of eight places in the series or a third of the length of the column.

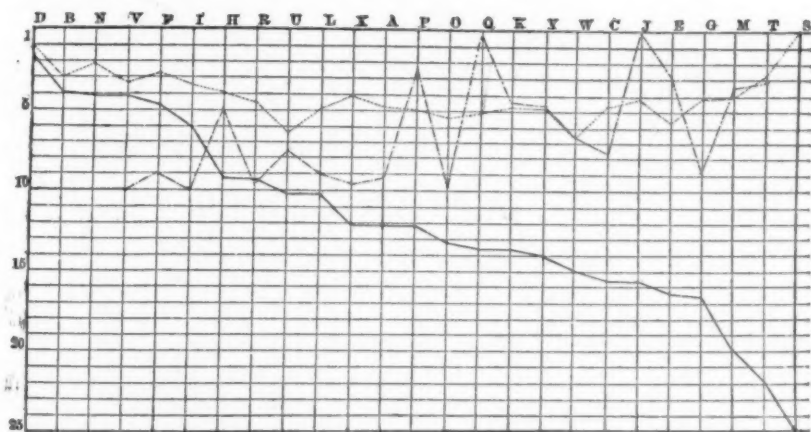


Chart II. Continuous line represents position of each letter in scale of certainty; dotted line represents degree of variability; broken line represents degree of unanimity in answers.

The letters near each end of the series show a much lower variability than those in the central region. Except for this general relation, there seems to be no connection between variability and grade of certainty. W and U are the most variable

letters, E and O are next in order. The dotted line in Chart II. shows the variability of each letter, the unbroken line representing the position of each in the scale of certainty.

TABLE III.—AVERAGE VARIATION OF EACH LETTER.

ALL.	PSYCHOLOGISTS.	ALL—EXCLUDING PSYCHOLOGISTS.
A 4.7	A 3.9	A 5.0
B 3.0	B 1.5	B 3.5
C 4.6	C 4.1	C 4.3
D 1.0	D 0.7	D 1.1
E 5.6	E 3.8	E 4.9
F 2.8	F 2.6	F 2.8
G 4.1	G 4.0	G 4.1
H 3.9	H 4.1	H 3.9
I 3.5	I 3.1	I 3.7
J 4.1	J 4.0	J 4.3
K 4.7	K 4.4	K 4.3
L 4.9	L 3.5	L 5.6
M 4.0	M 4.4	M 3.9
N 2.1	N 2.5	N 1.8
O 5.4	O 4.7	O 5.7
P 4.9	P 5.1	P 4.7
Q 5.1	Q 4.9	Q 5.1
R 4.5	R 4.1	R 4.8
S 0	S 0	S 0
T 2.7	T 3.1	T 2.6
U 6.3	U 5.5	U 6.7
V 3.4	V 1.5	V 4.0
W 6.5	W 5.8	W 6.8
X 4.1	X 4.7	X 3.9
Y 4.8	Y 3.8	Y 5.1
Average for all letters 4.	Average for all letters 3.6.	Average for all letters 4.1.

3. *Answers Positive and Negative.*—Table IV. gives the answers positive and negative to the various questions. Only the 'totals' need concern us at present. In the 'no opinion' column are tabulated those cases where questions were given the rank of 25, *i. e.*, placed upon a par with S. Something of an analysis of this table will be given later. The degree of unanimity in the replies to a given question is measured by the difference between the number of positive and negative answers. The greater the unanimity, the greater, of course, is this difference. In Chart II. the broken line indicates the degree of unanimity. On the first six questions, of course all are agreed, the negative answers to F ("Is the earth round?") being doubtless due to a wrong interpretation of the word 'round.' One

would, perhaps, be at first inclined to expect the degree of unanimity to be represented in this chart by a descending curve of reasonable uniformity; in other words, that there would be most disagreement in answering those questions upon which our opinions are least firm. But this is by no means the case. Upon H, which stands seventh in the series, there is more division of opinion than upon half of the questions which follow it, while G, which is answered 'yes' almost unanimously, stands fourth from the last in the order of certainty. Compare, also, P and Q with C and G.

TABLE IV.—ANSWER TO QUESTIONS.

	Yes.					No OPINION.					No.				
	♂	♀	Psychol.	Others.	Total.	♂	♀	Psychol.	Others.	Total.	♂	♀	Psychol.	Others.	Total.
A	63	33	28	68	96					0	2	2	1	3	4
B	65	35	29	71	100					0					0
C	10	1	4	7	11	2		2		2	53	34	23	64	87
D	65	35	29	71	100					0					0
E	23	10	8	25	33	3	3	3	3	6	39	22	18	43	61
F	62	33	28	67	95					0	3	2	1	4	5
G	61	32	29	64	93	1	1		2	2	3	2		5	5
H	50	24	22	52	74					0	15	11	7	19	26
I	65	35	29	71	100					0					0
J	29	20	8	41	49	1		1		1	35	15	20	30	50
K	20	6	11	15	26	3		3		3	42	29	15	56	71
L	63	32	29	66	95					0	2	3		5	5
M	35	27	16	46	62	7	4	5	6	11	23	4	8	19	27
N	65	35	29	71	100					0					0
O	64	34	29	69	98	1	1		2	2					0
P	27	10	14	23	37	1	1	1	1	2	37	24	14	47	61
Q	36	12	17	31	48					0	29	23	12	40	52
R	64	33	29	68	97					2	1			1	1
S					0	65	35	29	71	100					0
T	16	8	9	15	24	14	7	8	13	21	35	20	12	43	55
U	6	5	3	8	11	2	1		3	3	57	29	26	60	86
V	65	35	29	71	100					0					0
W	7	7	2	12	14	4	2	4	2	6	54	26	23	57	80
X	64	34	29	69	98					0	1	1		2	2
Y	50	23	22	51	73		1		1	1	15	11	7	19	26

There would thus seem to be beliefs which nearly all of us hold, but which we hold with far less certainty than we do some other beliefs concerning which there is much disagreement. The speculative nature of its subject matter or its remoteness in time or space may give to one of our beliefs a low degree of intensity

while our possession of the same data or the same prejudices may lead us, nevertheless, to hold it unanimously (A, O, C, G). On the other hand, much of the disagreement in the answers to some of the questions (*e. g.*, H, P, Q) is probably more apparent than real, being due to a misunderstanding of the intended meaning of the words.

Another relation of interest between the character of the answer and the position of the question is to be found in the fact that, in nearly all cases where there was a marked difference between the number of positive and negative replies to a given question, those siding with the majority expressed their conviction with a considerably higher degree of certainty than those siding with the minority. In Table V. these letters are given, with the average position in the series assigned to them by those answering 'yes' and 'no,' respectively. That reply is italicized in each case which was given by the majority of subjects.

TABLE V.

A <i>yes</i> 12 —no 13.9	L <i>yes</i> 10 —no 15.2
C <i>yes</i> 17.4—no 15.0	M <i>yes</i> 17.2—no 20.9
E <i>yes</i> 16.7—no 15.4	P <i>yes</i> 11.6—no 12.0
G <i>yes</i> 16.5—no 15.8	T <i>yes</i> 21.4—no 20.7
H <i>yes</i> 8.5—no 10.9	U <i>yes</i> 16 —no 8.9
K <i>yes</i> 15.5—no 12.4	W <i>yes</i> 17.4—no 13.7
	Y <i>yes</i> 13 —no 16.3

4. *Arrangement of Series in Relation to Sex.*—My subjects numbered 35 women and 65 men, but, as only one of the women was a special worker in psychology, it seemed fairer to compare the women not with the men as a whole, but with the men excluding the psychologists. Thus the sexes could be compared free from other complications. As we shall see later, the training of the special students in psychology led them to depart more from the untrained of both sexes than the latter did from one another. In Chart I., *b* is the average series for the women, *c* that for the men, excluding psychologists, numbering 37. As will be seen from the chart, the following letters are given a higher position by the women than by the men, only difference of $\frac{4}{10}$ or over being counted: F, H, U, A, O, X, P, W, Q, E, M. The letters given a higher position by the men than by

the women are: D, B, R, Y, J. It is interesting to note that the former list contains the only five questions out of the twenty-five having an ethical or religious import, namely: H, A, O, P and E. The position given to a question in the scale of certainty is an index of the force with which it appeals to one's interest. Thus, also, Y and J are questions which one would expect to interest men more than women. The higher position given to D and B by the men points to the latter having given a more careful consideration to the task of arranging the questions, for only a thoughtless person would place either D or B much below the top of the series, and this was evidently done by a greater proportion of women than of men. R on the men's side finds its counterpart in F on the women's. This position can have no special significance in either case.

Considering the number of positive and negative replies in relation to sex, we find from Table IV. that C, E, H, K, P and Y are given a considerably greater proportion of negative answers by women than by men,¹ while M, U and W are given a greater proportion of positive answers. For the other questions, there is little if any difference to be found in the proportion of answers given by the two sexes. The condition in regard to E, H, P, Y and W is perhaps as we should expect; the case of the other letters is less intelligible.

5. *Scale as Arranged by Special Students of Psychology.*—Scale *d* in Chart I. represents the average arrangement of the questions by the 29 instructors and special students in psychology. A glance shows that this scale differs widely from *a*, *b* or *c*, and differs from all of them on a point on which they agree with one another. The letters are more widely and uniformly distributed throughout the column. There is less concentration at the middle. Such concentration in *a*, *b* and *c* results from each of the letters so affected having been subject to wide variation up and down the column, so that in the average they all tend to fall into a mean position in the series. It indicates that a larger part has been played by chance in the arrangement of the questions. Thus there seems to be a greater approach to unanimity among that group of my subjects who

¹ Here all the men, 65 in number, are included.

were best qualified to perform the experiment satisfactorily. We shall find further proof of this below.

The scale representing the psychologists' records differs more from that of the other men and the women than these do from one another. The average difference of each letter in the former case is 1.4, the latter (men and women) differing from one another by an average of only 1.

In the psychologists' scale we find the following letters given a higher position than in that of the other men (only differences of $\frac{1}{10}$ or over being counted): V, B, F, I, L, U, H, P, Q, X, O. The following are given a higher position by the other men: N, A, K, Y, C, W, E, G.

V and B naturally appeal to the psychologist, the first dealing with one's own state of consciousness, the second with the existence of other minds. L and U deal with scientific hypotheses, while H, P and Q are problems of philosophy. X concerns the future development of the human mind. F, I and O do not seem especially to belong here, except, perhaps, for the reason alluded to in the preceding section for the position of D and B.

In the second list, N, Y, C, E and G are concerned with data of a more practical and tangible sort, N, C and G being largely founded on historical evidence and Y upon the direct experience and observation of many men.

For the answers 'yes' and 'no' given by this group of subjects, the reader is referred to Table IV.

It is significant that the variability in position of most of the letters is considerably less for the psychologists' series than for the others. The average variation of all the letters is for the former 3.6, for the latter 4.1 (Table III.). Here, then, is further illustration of the fact that those subjects who gave the matter most careful study have arrived more nearly at a standard arrangement of their beliefs. This is strong evidence that the great differences which we observe in the order given by different men are due not so much to actual differences of belief as to imperfect introspection.

6. *A Second Trial Made by the Same Subjects.*—This was done by nine of my subjects. The second trial was made, on the average, twenty-four weeks after the first. In the cases

where I interrogated the subjects, they denied emphatically having been consciously influenced by their first performance of the test. My object was to determine how much stability there was in this attempted gradation of one's beliefs and how much was merely due to caprice. The result of the test was most gratifying. The average departure in position of each letter in the second trial from that in the first was only 2.3. As we learned above, the average departure of all the letters in each man's arrangement from the general average of all other men was 4.

V. A LATER EXPERIMENT.

The length of the series of twenty-five questions rendered the performance of the experiment rather a long task. Again, the nature of most of the questions made it an impossible test for children. Accordingly I selected from the former set five of a very simple nature (A, N, O, V, Y), converted them into propositions and directed my subjects to arrange these in their order of certainty. Thus:

A [O] The most honest man I know will be honest ten years from now.

B [V] I am awake at this moment, that is, I am not merely dreaming.

C [A] The people of the world are becoming better.

D [Y] It would benefit the majority of young men to attend college.

E [N] George Washington was a real person.

My subjects ranged in age from fourth-grade pupils to college seniors. I received over two hundred records, of which I found it possible to utilize 187. To simplify computation I rejected those papers, forming a small proportion of the whole, in which any two questions were given coördinate rank. In presenting the results I have divided the subjects into two groups. The first includes the college students, 84 in number, with an average age of a little over 21 years. The second includes the grammar and high-school pupils, 103 in number, with an average age of a little over twelve years.

Table VI. gives the average arrangement of the series by all the subjects, also by sexes and by groups. Along with

them I have indicated for comparison the rank of the same five letters in the first experiment.

TABLE VI.

Average for All (187).	♀ (81)	♂ (106)	Older Group (84).	Younger Group (103).	Arrangement of same letters in first exp.
V 1.7	V 1.8 N 1.8	V 1.6	V 1.7	V 1.7	N 4.1
N 1.9	Y 3.4	N 1.9	N 1.9	N 1.9	V 4.2
Y 3.4	A 3.8	Y 3.3	Y 3.3	Y 3.4	A 12.1
A 3.7	O 4.2	A 3.7	A 3.9	A 3.6	O 13.1
O 4.3		O 4.4	O 4.2	O 4.4	Y 14.0

It will be seen at once that the grading given in my second test does not agree with that in my first. Again, it will be noticed that the older and the younger groups have given the same order of arrangement and also that the boys and the girls have given the same order except for the latter making the first two propositions coördinate. Here we find a greater constancy than in the first experiment. Table II. shows at once that, of the three groups of subjects recorded, no two agree as to the relative position of the five letters under consideration. Doubtless the greater simplicity of the second experiment is accountable for this difference.

VI. SUMMARY.

Our results, then, are, in brief, as follows:

I. Two-thirds of my subjects (in the first experiment) considered it possible to arrange their beliefs in a graded series which was fairly satisfactory to themselves at the time.

II. On comparing the average arrangement of the questions for men and women, certain differences appeared which seemed to be characteristic of the two sexes.

III. Even more characteristic differences were found on comparing the arrangement made by the trained psychologists with that by those who had had no special training in this subject. The former were found to have made a much closer approach to a standard order of arrangement than the latter. This implies that a great deal of the recorded difference of belief was due to defective introspection. The subjects have re-

corded, not what they believe, but what they think they believe, and, of course, the trained psychologists have been better qualified than the others to discover what their true beliefs are.

IV. A second trial was made with some of the same subjects after time enough had elapsed to allow of their forgetting their first results. This afforded further evidence that a graded arrangement of one's beliefs may be made which is not merely due to caprice, but represents something real.

A MIRROR PSEUDOSCOPE AND THE LIMIT OF VISIBLE DEPTH.

BY PROFESSOR G. M. STRATTON.

University of California.

In the course of an interesting review of recent work on the visual perception of depth,¹ M. Bourdon comes to the question why the heavens seem the particular distance above us that they do. In substantial agreement with Lipps, he explains the matter as arising from the limitations of binocular vision. There is a limit beyond which all objects appear equally distant, so far as immediate stereoscopic appreciation of their positions is concerned; so that the stars cannot be directly felt as farther than the maximal range of binocular effectiveness. This maximum, therefore, whatever it may be, fixes for us the distance of the vault overhead. Taking an angle of 60" as the threshold for the perception of spatial differences in the visual field and 65 mm. as the average interocular distance, he computes the range to be about 220 meters, and believes that this agrees fairly well with the apparent distance of the sky.

By a similar computation, after experiments in discriminating the distances of objects less than a meter from the eye, Helmholtz² gives '240 meters, or more,' as an estimate of the extreme distance at which an object might still appear in stereoscopic relief against a background infinitely remote.

These numbers were doubtless intended only as rough approximations of the actual limit. But a more direct examination of the fact inclines me to believe that they can hardly be accepted even in this spirit, and that the method by which they were made must in some way be open to objection.³

¹ Les résultats des travaux récents sur la perception visuelle de la profondeur. *L'année psychologique*, IV., 390.

² *Physiologische Optik*, 2d ed., pp. 790, 791.

³ Professor Le Conte in putting the limit at 'perhaps a quarter of mile'

The problem, it seems to me, can be attacked by means of the pseudoscope, and perhaps most conveniently and successfully when in the form shown diagrammatically in the accompanying figures. A box provided with two eye-holes (near L and R in Fig. 1) is open on the side opposite these holes. In the box are two perpendicular mirrors (M and N) inclined at a horizontal angle of 45° to the line of sight. Each of these mirrors is rigidly held in a small frame (for simplicity's sake, not indicated in the figure) which can be slipped to the right or left in the box and, if need be, turned slightly so as to vary the inclination of the mirrors. In a well-constructed instrument the entire movement of the mirrors would be delicately controlled by thumb-screws. The mirror M faces outward and to the right; the mirror N , inward and to the left.

It is apparent that when the mirrors are in the position shown in Fig. 1, the left eye is in direct view of the scene along the

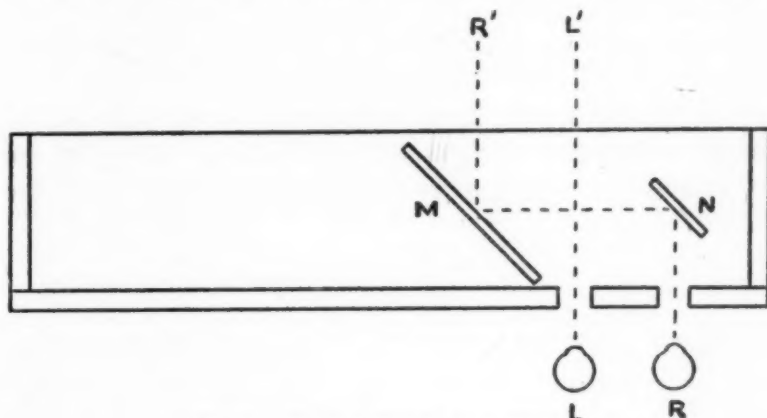


FIG. 1. Normal Pseudoscopic Vision.

line LL' while the right eye receives its light along the doubly reflected line RR' , so that its view of the scene is practically from a point to the *left* of the left eye. The relative points of view of the two eyes are thus interchanged and a vivid pseudoscopic effect results. With a little care in adjustment the dis-

(*Sight*, 2d ed., p. 163) comes nearer the mark, although I believe that this, too, is short of the true figure. He does not state the method by which he reached his result.

tance between R' and L' can be made equal to the interocular distance, and the difference in parallax for different objects remains the same as in normal vision. But the instrument also permits a wider separation of the lines R' and L' by carrying the larger mirror farther to the left (as in Fig. 2). This ar-

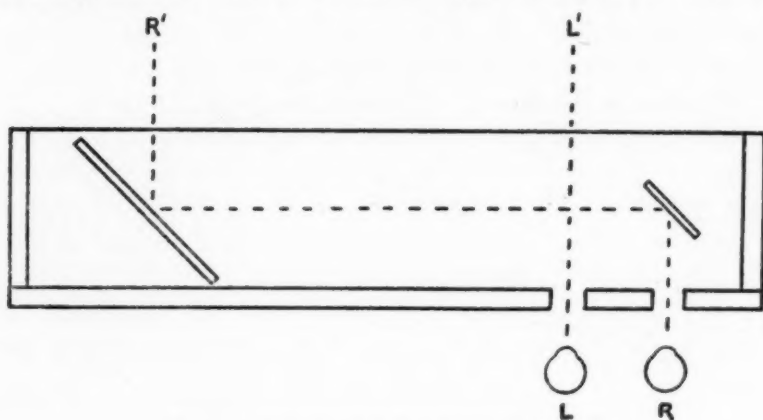


FIG. 2. Exaggerated Pseudoscopic Vision.

angement increases the parallax, and gives as a consequence a marked accentuation of the pseudoscopic effect. If, again, the smaller mirror on the right be moved so as to come before the

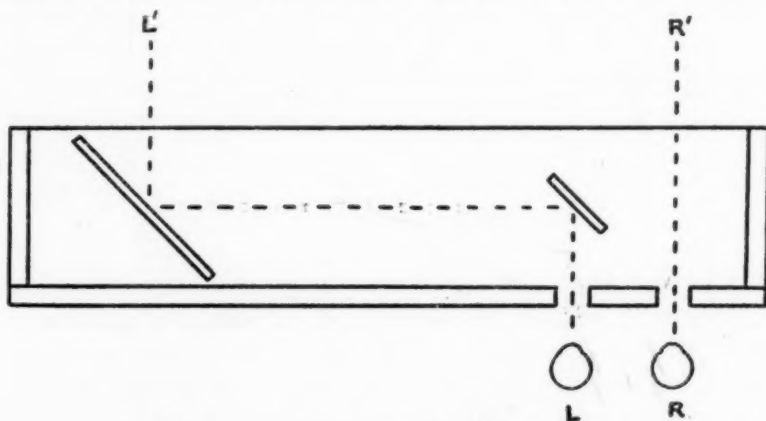


FIG. 3. Exaggerated Stereoscopic Vision.

left, instead of the right, eye (as in Fig. 3), L' and R' are then in the same relative positions as the eyes to which they respec-

tively lead; the pseudoscopic effect consequently disappears, and the instrument becomes what has been termed a 'telestereoscope,' giving an abnormal relief to objects in the foreground and carrying the stereoscopic effect out into the distance which normally seems 'flat.'

The advantages of this instrument over the ordinary pseudoscope which makes use of reversed stereoscopic photographs are obvious. In this, as in the Wheatstone pseudoscope, one looks directly at the objects themselves and not at their dull copy. There is, however, no right and left reversal of things, such as the Wheatstone instrument produces, and one can readily get a much larger field of view than ordinary prisms give. Besides this, an indefinite range of variation of the apparent interocular distance is possible for both pseudoscopic and stereoscopic vision, and consequently an elasticity in experimental use which neither of the other forms permits. For nice experiment with objects very near at hand some correction might be introduced by lens, or otherwise, so as to compensate the slight inequality of accommodation in the two eyes, resulting from the greater distance which the light reflected in the mirrors has to travel, compared with the light which comes to the other eye direct.

In applying this contrivance in the present case, the distance between R' and L' was made equal to that between R and L , and in other respects the arrangement was that shown in Figure 1. The landscape seen under these conditions shows pseudoscopic reversals, but not so often an apparent change of convex into concave objects, and *vice versa*, as a transposition of the relative distances of objects from the observer. A tree, for example, between the person and a background of other trees may now seem to lie beyond those trees and to be seen through them. There is a distance in the landscape, however, beyond which such transpositions are not noticed, so that the foreground alone shows the pseudoscopic effect, strictly speaking. But where two objects actually suffer such a transposition, one may safely assume that at least the nearer of them is still within the range of binocular perspective. For the transposition is brought about merely by the reversal of the usual binocular differences; and if the objects were so far

away as to make their distances binocularly indistinguishable, then the pseudoscope ought to leave them indistinguishable, and no reverse perspective would result. Where the instrument does produce an alteration of perspective it is evident, therefore, that the objects have an effective binocular difference, or, in other words, that at least one of the objects is inside the limit of stereoscopic vision.

But the pseudoscope is effective even beyond the region where actual reversals take place. In this farther zone, though, it gives no pseudoscopic effect in the ordinary sense of the term, but merely saps the stereoscopic life of the scene and leaves it with only the perspective that a skillful painting might have. When the instrument is removed, these more distant objects instantly show a clear stereoscopic relief, which is lost the moment the apparatus is again put before the eyes. The rapid alternation which is thus possible makes one for the first time unmistakably conscious of the presence of real binocular effect. There is, however, a distance at which this perceptible difference between the normal and the pseudoscopic view is lost. Things in this outermost zone look the same—absolutely flat—whether we look at them in the one way or the other. But where there is a rhythmic loss and reappearance of stereoscopic relief, according as the pseudoscope is put to the eyes or taken away, it must be that we are still within the range of true binocular influence. If we were looking quite beyond that range, there could not possibly be the alternation of flat and depth effects which was noticed under these conditions.

The limit beyond which all distances become binocularly indifferent can be roughly approximated in this way. Much more careful series of experiments than I have yet carried out would be necessary, however, before one could speak unguardedly, even as to the mere general position; but I feel certain that the alternation just spoken of is still perceptible in objects 580 meters distant and seen against a varied background of wooded plain several miles away. My own experience has been confirmed by three careful persons—the only ones who were called on—one of these being Professor Le Conte, who very kindly consented to make the observation. Tests were

also made to see whether we were not being tricked into an illusion of stereoscopic perspective by the mere added brightness which the scene showed when both eyes received the light direct, in contrast with the pseudoscopic view where one eye received an image slightly dimmed by the absorption of light in the mirrors. Instead of removing the pseudoscope and looking at the scene in the usual way, an additional set of mirrors was placed in front of the instrument, so that the left eye, too, received a doubly-reflected image of the scene, but so that the line of sight L' in Figure 1 was carried over the full interocular distance (but no more) to the left of R' . As soon as the lines of sight were thus restored to their normal relation the stereoscopic perspective returned, although in this case the scene was dimmer than the simple pseudoscopic view. If the supposed perspective had been an illusion due merely to the increased light, and not to binocular differences, it would, of course, have failed to appear under these special conditions.

While this more direct method of determining the range of binocular effect seems to me to be important, yet the actual result would after all be but the substitution of a new number for the old, were it not for certain theoretical consequences which the new number implies. The interocular distance in my own case is between 65 and 66 mm., so that in the two retinal impressions of an object distant 580 meters and projected on a background infinitely remote there would be an inequality amounting to less than $24''$. Yet under very favorable conditions, differences of position less than $50''$ can no longer be consciously discriminated;¹ and even under the most favorable conditions—the discrimination of fixed stars²—points have never been distinguished when separated less than $30''$.

If my present results are at all trustworthy, they would imply, therefore, that the spatial character of the presentation may be perceptibly altered by the presence of differences so minute as to be of themselves entirely inappreciable. The limit of conscious discrimination of angular differences consequently gives no exact basis for computing the limit of conscious binocular effect; on

¹ Helmholtz: *Physiologische Optik*, 2d ed., p. 259.

² Hooke, cited by Helmholtz, *ibid.*, p. 256.

the contrary, this effect may be produced by differences which elude our introspective scrutiny, or which are subconscious, if we wish to use the term in this sense. Those who hold that direct introspective analysis must give the sole and final word as to the constitution of a mental state might, I imagine, still maintain that the stereoscopic aspect of the perception cannot be due to factors which our inner sense is unable to report; that if these elements are indiscernible it were better to deny that they exist. It would seem to me more reasonable, however, to hold that the causes of this peculiar relief are the same wherever it appears, they being spatial differences in the two visual images, and perhaps, to some extent, differences in the orbital sensations when different parts of these images are superimposed; and that these motives are of themselves directly perceptible when at their best, but in their subtler phases they escape our introspection completely, although still capable of producing an effect which *is* introspectively apparent. This persistent efficiency, in consciousness, of motives which have become subliminal seems to me the interesting fact which the present experiment illustrates.

DISCUSSION AND REPORTS.

THE PSYCHOLOGY OF THE WILL.

A recent article by A. Pfänder from the Psychological Seminary of Munich offers a detailed criticism of several modern attempts to analyze the inner experience of will-action (*Das Bewusstsein des Wollens*, *Zeitschrift für Psychologie*, Vol. 18, pp. 321-367). It was my attributed duty and, at the first reading, also my intention to write an objective report of this careful essay; but at the second reading I changed my intention and the editor was kind enough to reconsider after that also my duty. An article, the arguments of which follow, critically, the arguments of others, can hardly be reported without repeating not only the critic's, but also the criticised discussions, and that would lead us too far into detail. Thus it may be sufficient to report that the author rejects every analysis which tries to exclude a special will element, that is, which reduces the will to a complex of sensations. The final word leads to the theory of Lipps, who gives to the will its fundamental place.

Instead of a further abstract of the paper, it may be allowed to me to mention a few points in the defense of my little book on the will (*Die Willenshandlung*, 1889), the criticism of which makes up the first half of Pfänder's paper, the second half being devoted to James, Kuelpe, Ribot, Baldwin, Wundt and Lipps. I wish to mention a few points in which Pfänder misunderstands my meaning, and, above all, I wish to add a general word about the whole question, a word which I have had for a long time on my lips, especially since Mr. Seth and others have chosen that first essay of mine as the whipping-boy of physiological psychology.

I have tried to show that we can decompose the psychical facts of the will into elements which are by principle coördinated with the elements of the idea and that the most essential rôle belongs to the fact of anticipation. The fact that an end is anticipated before it is reached by our own activity makes up a chief characteristic of the will, as only that anticipation of the end allows associations about its consequences and thus the stopping of the possible action through the inhibitory function of the association. I showed how also in the case of inner

will-action the result is determined by the anticipation, and how the so-called innervation-feeling is in the same way the anticipation of the movement sensations which will result from the action.

Pfänder first denies some of my facts. He does not allow that, if we try to remember an idea, the idea itself precedes the inner action which brings about the reproduction. Of course, if we try to remember a name, the name itself is not in consciousness, but, as I said in my book, an x which is given in such relations to other ideas that it can be only the sought name. Pfänder says: The idea is present or is not present; an acknowledgment of such an x as substitute destroys the theory. He does not see that such substitute, which is of course quite different from the idea itself in regard to its sensational structure, is perfectly identical with it in regard to its associations, and that for my whole theory this side alone is essential. If x can awake and inhibit the same associations and actions as the concrete idea, its existence is an anticipation of the idea in the only respect in which it was in question, namely, as the center of functional relations. I think he is more correct in another point. He says that the innervation-feeling which accompanies inner activities is not an anticipation of later actions, because such actions are not produced. That is true, but I should say, it does not militate against my theory of innervation-feelings because that accompaniment of inner activity is hardly felt as feeling of innervation, it is felt merely as feeling of activity which only by its fusion with the characteristic succession of ideas becomes part of a will act.

Pfänder misinterprets, secondly, some parts of my discussion by taking the conception of anticipation too narrow. I did not mean only that the memory image comes before the perception, but that it comes before the perception with a feeling of relation to the future. It is anticipated as something which will be realized in the future. This feeling of reference to the future includes not the slightest volition, it is merely the feeling that preparation for its appearance is still possible; in other words, it is the sensational accompaniment of a special set of preparatory motor adjustments. In the same way it is no objection that our will to act can exist without the later realization of the anticipated motion; we have then will, but not a will-action. In such a case we have the anticipation of an effect thought as realized in the future and the anticipation of the feeling of the action which brings about such effect together with the inhibition of all ideas which would produce antagonistic actions; whether under these circumstances the action really results or is stopped by an outer obstacle, is without con-

sequences for the feeling of volition. I did not pay much attention to this case, as my book was especially devoted not to the will, but to the willed action. The deciding point remains also here that the active will can be decomposed into a system of passively felt, not actively willed elements.

But just upon this general point is the real bearing of Pfänder's whole criticism; all the previous special discussions are secondary. If all the claimed sensations were given together by outer influences, for instance by electrical stimulation of the different nervous parts, we could never understand how they can form that consciousness of activity which characterizes the real will. A man in such a case, Pfänder says, would feel that something is happening to him, that a complicated surprising cramp has attacked his body, but he would not believe that he himself is doing something. It would seem perhaps a sufficient answer that as long as such artificial synthesis is not made experimentally, it is not essential whether we can understand the result or not; those who have never heard that a stereoscope really exists, would be probably not less skeptical about the claim that the perception of two flat pictures gives the impression of one plastic object.

But such an answer would be misleading. Pfänder's criticism, which coincides here fully with that of Seth and scores of others, is not to be rejected because we can show that our psychological analysis is right, but above all because we have never claimed for our analysis what they criticise. My critics ought to show that my analysis of the psychological facts of the will is incorrect, and, instead of that, they show only that the analysis of the psychological facts is not a description of the real will. But who in the world has pretended that it is? I analyze the contents of consciousness which I find as soon as I transform the will into a complex of psychological phenomena, and they cry behind me: Stop thief; the real will is primarily not given as a content of consciousness which you find as describable object but as something which you must feel and will as your active function. Of course, such active function it is; only as such—it is not a phenomenon and therefore not describable and explainable, and if you want it as object of psychology, that is, of the science which describes and explains mental life, you must transform mental life into a set of objects and substitute in that service the psycho-physical personality for the real center of subjective functions.

To quote my own words from a recent paper, I may say once more: "As soon as the psychologist enters into the study of the will, he has absolutely to abstract from the fact that a complicated substitution

is the presupposition for his work. He has now to consider the will as if it were really composed of sensational elements and as if his analysis discovered them. * * * There is nothing more absurd than to blame the psychologist because his account of the will does not do justice to the whole reality of it, and to believe that it is a climax of forcible arguments against the atomizing psychology of to-day if philosophers exclaim that there is no real will at all in those compounds of sensations which the psychologist substitutes. Certainly not, as it was just the presupposition of psychology to abstract from that real will. It is not wiser than to cast up against the physicist that his moving atoms do not represent the physical world because they have no color and sound and smell. If they sounded and smelled still, the physicist would not have fulfilled his purpose." (*Atlantic Monthly*, May, 1898, p. 613.)

Of course, this may appear as a postscriptum. But my critics have no right to quarrel with me; I have never hidden my views and I have not essentially changed them. I have not only my students as witnesses that I have for many years characterized the rôle of psychology in this way, but I can call the little book itself to the stand. To be sure, I have always held the opinion that a monograph on a special subject has not the duty to report the author's views of all other things in the world. When I wrote an essay on the will as psychophysiological process, I did not feel obliged to discuss also the will in so far as it is a factor in the real world. I thought it sufficient to emphasize in the beginning that I was there not dealing with the will in so far as it is object of epistemological, metaphysical, ethical and practical reality, I wished to consider it only in that unreal transformation in which it belongs to psychology. To make my meaning perfectly clear, I ended the whole book by the words: "The will is an explainable complex of sensations seen from a psychophysical standpoint, but the deepest mystery seen from the standpoint of metaphysical reality." How much emotion my friends would have saved if they had taken the trouble to read not only the half of the sentence, but the whole!

I confess I should not write to-day the closing sentence as I did there ten years ago. I should avoid calling the ultimate reality a metaphysical one as there is no other reality while the world of physical and psychical phenomena is unreal. And I should still less call it a mystery, as this expression means that it is unexplained while it ought to be explained; I should say to-day that it is unexplained and unexplainable only in so far as the categories of explaining sciences, that is of physics and psychology, are applied to it, but, as the real will be-

longs to a subjectifying system to which the categories of the objectifying sciences do not apply, the question whether it can be explained does not come up at all. It is beyond causality, as it is beyond space or weight. An explanation would have no meaning for it, it must be interpreted and appreciated, not described and explained, but it is not, therefore, mysterious. I should thus prefer to close my book with the words: "The will is a describable and explainable complex of sensations if it is thought as transformed into a psychophysical phenomenon; this analysis, on the other hand, cannot say anything about the real will which belongs to the primary world, the more as this transformation of the real world into a describable system is itself a function of the real will."

Perhaps my critics would say here, that this escape to epistemological questions does not help the fate of my theory, as even then, when the task of the psychological analysis is reduced to this secondary treatment, we have to debate whether the elements of this unreal world are sensations only, that is, elements of ideas, or also volitions which cannot be coördinated with ideational elements. But I am convinced that even in this point the matter has to be handed over to epistemology, and all the psychophysical family quarrels about the muscle sensations and so on do not count much till we understand what we intend in general by our psychophysical research. We may debate about the ways which lead to the goal, but it is meaningless to discuss whether we ought to approach the goal or to depart from it.

Is it really only a specialistic caprice when some psychologists try to decompose all mental life into such elements only as are possible elements of ideas? Is it not rather the mere consequence of the presuppositions which constitute psychology? To be sure, in the detailed work, it is not necessary that the specialist remains always conscious of the epistemological purposes which give to his science logical value and meaning, but if he is doubtful about the general direction, he must look for his orientation indeed to the philosophical principles.

Psychology has nothing to do with interpretation and appreciation; it seeks to describe and to explain mental phenomena. It presupposes, therefore, that mental life is by principle describable. Description demands decomposition into elements and fixation of the elements for the purpose of communication. There is no description without communication, but mental states are never, and under no circumstances, directly communicable. We can awake and suggest mental facts in others, but we can never share a mental fact with others. Directly communicable is only the physical world which is the world

of common experience. We can communicate mental states, therefore, only indirectly, by connecting psychical experiences with the physical world; there was never a psychical fact which was communicated otherwise. This connection with physical facts for the purpose of fixation in the service of communication can pass, of course, through many stages, from the most indefinite popular reference to physical objects and situations to the most exact connection with measured physical processes, but there is no escape from the physical connection. It is, therefore, absurd to think that the relation of mind to body, of psychical to physical facts, comes in play only as soon as the explanation of the facts begins; no, the simplest description has its ultimate basis in the reference to communicable physical facts. In ordinary life we connect the whole mental state with a reference to a physical situation as a whole; in psychology we seek the determining physical connections for the distinguishable psychical parts, but the principle is the same.

But every science presupposes that its aim can be reached with ideal completeness, and it is its duty to transform the objects in thought till the ideal fulfillment of the purpose is at least thinkable. In its preparatory stages psychology finds plenty of possibilities from which it can select in connecting psychical and physical facts; for instance, the mental fact and its physical cause or its physical effect or its physical accompaniment and so on. Each of these possibilities has its importance for some stage of psychology, but no one can represent the final stage, as no one allows an epistemologically ideal connection. There exists only one connection between psychical and physical facts which is independent of empirical observations and of empirical confirmation, the relation between the psychical idea and the physical object which is meant by the idea. This relation stands high beyond empirical chance because it is based on epistemological identity; those two experiences are in reality one which has become double only from a twofold way of looking at it. The idea is thus the only mental state which can be communicated and described by a connection which is logically necessary. If all mental states were ideas, the description would be easy. On the other hand, a mental state which cannot be described after the scheme of the description of ideas can never be perfectly described.

Psychology of the feelings, emotions, judgments, volitions would remain thus on a lower level of description if one possibility was not open. The ideas which correspond to the physical objects show discriminable parts; these parts may be called sensations and the sensa-

tions stand thus to the factors of the physical object in the same logically ideal relation in which the ideas stand to the whole objects. Emotions and volitions are not ideas, but if they were complexes of elements which are possible elements of ideas, that is, sensations, then their elements would be describable and they themselves thus describable in terms of their elements. Emotions and volitions can be communicated only if they are complexes of sensations, and therefore, as psychology has its aim in describing all mental facts, psychology has the duty to transform these states till they are represented by a complex of possible elements of ideas. To the instinctive service of this duty the well-known theories about the structure of emotions arose, and in this service I wrote my book on the Will.

The leading aim of the book, which is for Mr. Seth an 'ingenious caricature,' is then just as valid for me to-day as ten years ago. Of course I see to-day many details, especially about the feelings, otherwise than in my first essay, and with this in mind I said once jokingly in print that it was the product of guileless adolescence. But if Mr. Douglas in a kind defense against Mr. Seth's attacks pats me on the back and repeats the excuse of my youth as if I had given up the principle of the treatment, I must decline the defense. I should change to-day details, but the principle that the will, as soon as it is transformed into a psychophysical object, must be thought as a complex of sensations, was independent of the bold aggressiveness of my 'adolescence' and is still my present belief while "I am grown peaceful as old age to-night."

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WHAT IS A PSYCHICAL FACT?

In the September number of the *Educational Review* Professor Münsterberg announces that "the psychical fact as such is for philosophical reasons just as undescribable as it is unmeasurable, since it is the object which by principle exists for one only and which remains, therefore, ever incommunicable." It will be remembered that in his articles in the *Atlantic* Professor Münsterberg used this theory of the nature of mental facts as the chief prop to his thesis that the phenomena with which psychology deals can not be studied or talked about under the headings time, space and energy. It seems time for some one to join issue with him on this epistemological question, one of vital importance to general method in psychology, especially since he

has a supporter in Professor Royce, who has maintained quite as emphatically¹ the individual nature of the mental fact as opposed to the social, communicable nature of the physical fact. So, though it needs a David to fight against either of these giants while "Hercules himself is not enough for two," I may be permitted to open the combat.

One is led to suspect Professor Münsterberg's conclusions by their very manner of statement. In these days one resents being told that a thing is incommunicable 'for philosophical reasons.' A thing may, as a matter of fact, be communicated, or it may not, and the way to find out whether it is or not is to look and see. So many things have been declared out of court 'for philosophical reasons' which the widening researches of matter-of-fact men have triumphantly reinstalled that any one may be allowed the liberty of appealing to observation of facts against any one's philosophical argument. A 'philosophical reason' may be unassailable by attacks like itself and yet fall easy victim to some newly-discovered fact. So also when one is told that an object exists for one only 'by principle,' one properly demands that the advocate frankly confess that he means nothing more than that as a matter of experience the object is found to exist for one only. Otherwise we shall suspect him of a dangerous approach to scholastic methods. We can agree or disagree with Professor Münsterberg only after reviewing the facts to see what does happen.

It is unfortunate that he does not designate a lot of examples of the thing 'psychical fact' about which we are to argue, for the real quarrel may be about the meaning of these words. We may not have the same realities in mind when we use them. He might retort to this suggestion that he could not designate such a lot of examples, because perforce such are incommunicable by him to us, indescribable to such an extent even that they can't be pointed out for recognition. This retort would, of course, be suicidal for Professor Münsterberg, for then he is as vain in his negative talk about the mental fact as we are in our positive, our measuring, describing and such. No one else can know what he is talking about or have in common with him the object under discussion. For him to write articles about psychical facts is then the height of absurdity.

Since he has not designated examples, we shall have to take up the question on several bases, according to several possible meanings of 'psychical fact.' My claim is that if it means for him the facts which we think about in connection with men but not with stones and trees,

¹ In his articles on 'Social Consciousness.'

the sort of thing we admire as courage and sympathy, pay for in college professors, statesmen, or inventors, the stuff we try to control in our children and students, his statement that a psychical fact is for one only, is uncommunicable, undescribable and unmeasurable, is false. If he means something other than this, I claim that there are mental facts, existing for a number of observers, describable and measurable, that knowledge about them is mental science, as knowledge about physical facts is physical science. The truth or falsity of these claims will be best established if in examining them we start from fundamentals. We will talk in idealistic terms, though any one may supply their equivalents.

The total of pure experience, of mere 'sciousness,' feeling or whatever we choose to call the naked being of the stuff which includes all, is divided into two great classes. The first sort of stuff (Class I.) is those sense-impressions which we commonly call things; the second sort (Class II.) is emotions, volitions, dreams, images, pleasures, pains, reasonings, general bodily feelings, etc. The stuff of Class I. may be looked at either as mere bare experiences situated in individuals as centers or collections of experiences, or it may be looked at as divested of its personal form, as a lot of facts common to us all, as the world of nature, as *things*. We will call the former the A aspect, the latter the B aspect. The A aspect, then, is this half of experience as it exists for itself; the B aspect is it as it exists for a multitude of thinkers. Now the question between Professor Münsterberg and ourselves is as to whether there is possible a similar division of Class II. If he gives to the words 'psychical fact' the meaning which I described a while ago, he must, to support his theory, deny that the stuff of Class II. can be viewed in any other way than the A aspect. He must claim that experience 'sciousness' in these varieties remains always pure, undisturbed, existing only for itself. This simple question of fact we may now settle.

John says to Tom, "Bill is ugly." Tom asks, "What made him feel ugly?" John replies, "He feels mad because he has a headache. Bill is always ugly when he has headaches." Now, Bill's madness in its occurrence as a part of Bill's stream of consciousness, Bill's madness to Bill while mad, is in the A aspect, but as a fact thought about, realized by John and Tom (and by Bill himself when he no longer *is* it, but *thinks about* it), it is in the B aspect as much as is the apple-tree in Bill's garden. It is a definite, describable fact, dated in time, is measurable as madder or less mad than other mad feelings, is a thing which all the world will have to make hypotheses and judg-

ments about. Yet it clearly belongs to Class II., is clearly not a psychical fact, has no judgments made about it by physics, or chemistry, or biology, or any physical science. John thinking of Bill's madness is not thinking of any bodily movements. He and all of us sharply distinguish the two classes; we say that Bill was mad, *for* his blood rushed to the face, or that he was mad, but did not in the least show it at the time. We say that we learned of his madness from its physical expression, that the madness goes with such expression. But we never confuse the fact '*feeling* of madness' with the fact physical cause or expression or parallel of madness.

When physicians are consulting together about means of curing some poor wretch of the delusion that he is being persecuted, they all have in common the mental fact 'A's thoughts of being persecuted' as a fact to make all sorts of scientific talk about. Yet surely they are not thinking of any moving matter, any physical thing. No one knows the physical cause or parallel of a delusion of persecution. And the expression of it, the poor man's sorry tale of his woes, the muscle contractions in his vocal organs, are, of course, not what the doctors are trying to stop. What they know, what they want to stop, what books on insanity are written about, are mental facts, facts of Class II., and they are still facts of the B sort, facts for the world of observing scientists.

It would be childish to continue bringing evidence of this sort, and it would have been childish to have said even what we have if one could be sure that Professor Münsterberg had not, in his zeal for the truth founded on 'philosophical reasons,' disregarded the commonest facts of his daily life. To be sure, the facts of Class I. are defined by space relationships in a way that the facts of Class II. are not, but one should not, because a mental fact lacks the added definiteness, descriptibility and precise communicability which space facts have, jump to the conclusion that it is entirely devoid thereof.

It is possible, however, that Professor Münsterberg might at the start take the stand differently and first divide experience into its A and B aspects. We have, that is, experience as being for itself, experience in its first mode, and experience as stuff for a number of observers to possess in common. His claim then is that 'psychical fact' is the name for the A sort and that psychology studies only such, that all the facts of the second sort belong elsewhere. If this be his position no one can deprive him of the right to give any name he pleases to the A aspect facts or to use 'psychology' as a name for the study of experience in that aspect. But one can show, as we have shown,

that in so doing he leaves in among the B sort a tremendous body of facts which physical sciences have not treated and cannot treat if they stick to the space facts, and by the same right that lets him call them *not mental facts* we can call them the opposite. I for one do not care about *calling* them anything. I simply demand, as a would-be teacher and thinker about human nature, the right to study them apart from physical science, without being pooh-poohed and told beforehand that 'by principle' they are not amenable to such study. Moreover, one can perhaps show that experience in its A aspect does not present facts for science at all, that in all the psychologizing he has done Professor Münsterberg has never made a statement of fact about any fragment of it.

In attempting to do this let us return to Bill's madness, but this time to Bill's madness to Bill while mad, to Bill's madness in the A aspect as it bubbles up in the wellspring of experience. Professor Münsterberg is correct in claiming that Bill's madness to Bill while mad, that any feeling as felt by the feeler, is undated, unmeasured in duration or intensity. But is he correct in reserving a sort of knowledge of it by the one single person, a sort of descriptive intuition which is to make psychology? Why stop after excluding judgments of date or measure? The fact is that the feeling as felt by the feeler is not thought about by him or any one else. It *is* he. He *is* it. He has nothing to do with it but *be* it. Only when he stops fully being it and partly watches it can he make *any judgments whatsoever about it*; and as soon as he makes *any at all, he can make judgments of date, time, comparison, etc., which are perfectly communicable*, for to make any at all the fact has to abandon its A style of being and enter the B aspect. So Professor Münsterberg's argument, if carried to its legitimate conclusion, affords the best of reasons not for claiming that the science of mental facts should treat only of facts in the A aspect, but rather for claiming that it has always left them severely alone. Let the *livers* live their lives, be their feelings. Let the psychologists make judgments about these feelings as known to them in common. So one is tempted to retort to Professor Münsterberg that the A aspect facts are 'by principle' such that they are not material for science or judgments of any sort.

The present writer, however, does not wish to press this theory of the nature of mental facts upon any one or to restrict method by it. He offers it only to keep Professor Münsterberg's theory from restricting method, and to encourage every one even in normal schools and child-study societies to go ahead making judgments about mental facts,

testing their mental judgments by experience, widening their acquaintance with human nature as much as they can, in full faith that they are making psychology. Very poor psychology it may be, very inaccurate and inconsistent and misguided. Very few successful guesses at fruitful hypothesis and very little verification of such will come from their work. But they can do work as good for the purposes of mental science as much of the work of naturalists has been for biology, as good possibly as much of the descriptive work of many professed biologists has been. Any attempt to improve the judgments of common-sense about any sort of facts may prove fruitful, and no one should be debarred from such attempts by being told that they are 'for philosophical reasons' doomed to failure. I know of no other way to tell what may be done in a science of mental facts than by trying, and even if my refutation of Professor Münsterberg's theory be a failure we have a right to try to refute him by attempting the science he denies.

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CONSCIOUSNESS AND THE UNCONSCIOUS.

The reading, before issue, of a forthcoming work on the general subject of 'unconscious' mental processes has revived the opinion, for some time entertained by the writer, that there is an aspect of the phenomena in question which has not always received adequate notice and the consideration of which is calculated to throw light on the whole problem. The view which I am about to present is, of course, not novel; in many respects it is as old as Leibnitz, although there are also many elements in that philosopher's treatment of the subject which I should not like to be understood to accept.

Psychologists are familiar with the phenomena which have given occasion for the belief in 'unconscious mental action' or the 'unconscious mind.' Examples of these are numerous both in the sphere of normal mentality (minimal perception, diffuse attention, the syntheses of perception, the 'retention of ideas,' cases of 'unconscious inference,' habit, ideo-motor action, etc., etc.) and in the less frequent and customary manifestations of mental life (extraordinary reappearances of ideas once apparently forgotten, the performance of mental operations with the attention directed elsewhere or even during periods of sleep, facts of the hypnotic state, etc., etc.). The tendency of the psychology of the day in explaining such phenomena is also well known.

Prominent psychologists of former generations ascribed many of them to some unconscious functioning of mind, to processes assumed to go on below the threshold of consciousness and yet to lie within the limits of the mental rather than beyond them. This is, moreover, the opinion of some of the leaders still. But, for the most part, recent attempts at explanation take a different direction, 'unconscious mental phenomena' being assigned to the sub-conscious, semi-conscious borders of the conscious field, or else being denied title to the conscious predicate altogether and relegated to the class of physiological processes on which consciousness depends.

There is, however, a certain ambiguity in the use of the words 'conscious' and 'unconscious' which leads to considerable error in popular thought, and which may conceal essential facts in the case even from the trained psychologist. For these adjectives may be used in application either to a given psychosis in itself or to its relations to other mental processes. 'I did it unconsciously,' 'He did not know what he was doing'—such phrases, strictly interpreted, do not imply that the action went on entirely outside consciousness, but that the consciousness *of* and *about* the act was reduced to a minimum, or was altogether wanting. Thus the psychosis becomes a disconnected unit and, not being brought into conscious correlation with other psychoses, finds no place in the web of mental life, so that if notice of the occurrence is given from an extraneous source, a basis is furnished for the misconception, that, in some mysterious way, the original process was unconscious and mental in one. For instance, the man who nibbles nuts or fruit after dinner while barely aware of what he does (*cf.* James, *Principles*, II., 522) is not acting unconsciously in the full sense of the word; he is acting (1) with the minimum of consciousness necessary to the performance of the act in question, and (2) the point here raised, with the least conscious connection of his act with the other contents of his consciousness at the time or with an absence of such correlation. A good example from the side of cognition is given in 'the novel of the summer,' where the hero in describing his first meeting with the heroine is made to say: 'I marked, almost without knowing, the rope of pearls that bound her throat (I had become a judge of jewels by being the possessor of so many).' The chief clause here is an accurate statement of psychological fact; the parenthesis adds one of the causes which may contribute to the production of the phenomenon. The general truth is that mental life is a thing of degrees not only in the scale of intensity, but also in the scale of complication. If the web is less compact than usual, some of the customary connect-

ing strands being absent or attenuated, there is danger that the partial loss will be confused with a total disappearance. But since at the same time the functions of consciousness are in some measure performed, the fiction of unconscious mental operations arises.

There is a partial recognition of this aspect of the matter in some of the other views of the subject. Thus it is true that certain 'unconscious ideas' are faint psychoses of momentary duration, which hence attract but little attention, including connective attention; it is true, again, that 'ideas' are called 'unconscious' when, perhaps, belonging to the class just mentioned, they fail to be remembered while sub-conscious or semi-conscious states are obviously such as are not brought into distinct correlation with others, especially with those at the time in the focus of consciousness. The aim of the present discussion, however, is not to dispute the value of these explanations, but to call attention anew to the numerous cases in which the lack of conscious correlation noted constitutes the principal element in the assumed unconsciousness.

If a name be sought for this class of phenomena, psychologists might avail themselves of the classical expression of Leibnitz and say that psychoses of this kind are perceived but not apperceived, that they are perceptions but not apperceptions. And this would be altogether the best designation for them were it not for the wide variation in the meaning of the terms perception and apperception, both in Leibnitz's day and since. In view of this difficulty, in view, also, of the importance of a clear realization of the principle of greater and less complication in consciousness, it has occurred to the writer that, perhaps, we might speak of psychoses or consciousnesses of the first power, second power, third power, or first potency, second potency, third potency, etc. Such phrases would at least have the merit of calling attention to the facts of the case, but their elegance as English and their freedom from misleading associations would raise different questions.

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A CASE OF RETARDED PARAMNESIA.

So many cases of paramnesia or false memory have been reported and studied that this phenomenon, which was once supposed to be very rare and indicative of insanity, is now regarded as quite common and altogether normal. It may be the same with a peculiarity which

I have observed in myself for a number of years, but, as I have seen no positive references to it, and as it may throw some light on the mechanism of paramnesia, I am impelled to the disagreeable duty of reporting a personal experience.

In ordinary paramnesia an event which is really happening for the first time in the experience of the subject appears to him to have happened before. The observation of the event is simultaneously accompanied by a vivid flash of memory which represents it as having occurred in all its details at some time in the past. This experience I am familiar with, but it is much more common with me for the false memory to appear not when the event first happens, but at some later time after it happened. For example, I am confronted with a scene which seems and is new to me. It passes from my thoughts, but at some later time when it is recalled to my mind, voluntarily or involuntarily, the impression comes to me suddenly and vividly that I have looked upon the scene twice in my life under the same circumstances, and I wonder why I did not recognize it when I saw it. Ever afterward the scene remains in my mind as a double memory. The sensation of the paramnesia occurs from a few minutes to several weeks after the real event, but only, if at all, at the *first* time I think of it. The recollection thus duplicated may be an incident I see or take part in, a view, a picture or a story. The last case in particular, as when sometimes after reading a story in a new magazine I seem to have read it twice, might appear to be susceptible of the usual explanation of paramnesia, that of an imperfect memory of something similar, were it not for the fact that the circumstances of reading the story seem to have been exactly the same in both cases. The longer the time that elapses between the event and its first recollection with the accompanying impression of paramnesia, so much the longer the apparent interval between the events as reported by the false and the true memories, but I am unable to say whether the ratio between the two intervals is really a constant or not. Several instances of retarded paramnesia often occur within a short time, and then months may elapse without such an experience, but I have never been able to connect them with a state of poor health or fatigue.

Diligent inquiry among friends has failed to give any other instances of a similar phenomenon, and the only case I have found in literature is a reference, in Ribot, 'Diseases of Memory' to Pick, *Archiv für Psychiatrie*, 1876, but, not having access to the original, I cannot say how far it is similar to mine. Such a case as this, if confirmed by others, will have to be taken into consideration in con-

structing a theory of paramnesia. It is inconsistent with Lalande's theory,¹ as well as those of Anjel and Jensen,² unless the memory plays the part of the original perception. The theory of imperfect reminiscence filled out by suggestion is allowable, but does not give a complete explanation.

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MEMORY AND ASSOCIATION.

I wish to make a correction and comment briefly on Miss Calkins' interesting report of a study of memory and association and comparison with similar experiments made by myself. In no instance did I give the number of words correctly placed *in order* in each group, but only the numbers placed in the *right column* or group, as the auditory or the visual; hence her comparisons are not quite correct, and her statement that the Wellesley results do not substantiate the conclusions that "the number of concretes 'recalled' and the number 'recalled in order' would be under ordinary conditions practically the same" is a misquotation and indicates misapprehension. I make no reference to the number 'recalled in order,' or to 'ordinary conditions,' but merely indicate that there was in this experiment practically no false recognition or placing in the wrong column of the concrete objects.

It is interesting to note how nearly the results of the two experiments agree. The better memory of the Wellesley students is probably due to the facts that the external conditions were more favorable and the students more interested in the experiment than the college students tested by myself. It seems to me very probable that the smaller advantage in favor of 'concrete' found by Miss Calkins is due in part to another fact than that mentioned by her, *i. e.*, she used pictures of objects while I used real objects. It would be interesting to know just how much difference this would make when all the other conditions are the same. I suspect that the order of increasing effectiveness would be names of objects, mental pictures of objects, real pictures, the objects themselves.

E. A. KIRKPATRICK.

¹ Lalande: *Des Paramnésies*, *Revue Philosophique*, XXXVI., 485. Reviewed by James, *PSYCHOLOGICAL REVIEW*, I., p. 94.

² Krapelin: *Arch. f. Psych.*, 400, in Parish: *Hallucinations and Illusions*, p. 280.

THE PSYCHOLOGICAL LABORATORY.

During the past quarter three papers have appeared that are of interest to all students of psychology.¹ They are so accessible that an abstract is scarcely needed, but some comments may be profitable, more especially if they lead to further discussion.

Professor Titchener describes the Cornell laboratory and the general needs and functions of a psychological laboratory with the skillful hand to which we are accustomed. The members of the Psychological Association who attended the Ithaca meeting know that he has an admirable subject. The Cornell laboratory, owing to its large resources and able management, may serve as an example. It is a pity that the trustees of our universities do not read *Mind* in order to learn that \$2,000 should be granted for the establishment and \$600 annually for the support of a laboratory. I do not know the resources of Chicago and California, but probably no other American or foreign laboratory (except Professor Wundt's, as the result of its longer history) has fared quite so well as that at Cornell. University trustees do not read *THE PSYCHOLOGICAL REVIEW* either, so there is no danger in saying that Professor Titchener's estimate of \$300 annually for current expenses seems to be rather extravagant.

As Professor Titchener asks for discussion and criticism I shall take up one or two topics. The large ground plan and the description of the laboratory show that, in addition to a lecture-room and rooms for the director and assistant, there will be, when the laboratory is complete, eight rooms. There are, or will be, a small workshop, two dark rooms, a reaction room, a room for the 'physiological processes underlying affective consciousness,' rooms for haptics, and for taste and smell, and large rooms for optics and acoustics. Now, I should find the laboratory more useful if the large rooms devoted to vision and hearing were each divided into two or three small rooms, and all the rooms named after the senses were called *x*. For research a large room cannot be used simultaneously for more than one purpose, and nothing seems to be gained by setting up all the work on vision, for example, in one room. In some years there may be several researches in prog-

¹ A Psychological Laboratory, E. B. Titchener, *Mind*, N. S., No. 27, July, 1898, pp. 311-331.

The Place of Experimental Psychology in the Undergraduate Course, F. C. French, *THE PSYCHOLOGICAL REVIEW*, Vol. V., No. 5, September, 1898, pp. 510-512.

Principles of Laboratory Economy, E. W. Scripture, *Studies from the Yale Psychological Laboratory*, Vol. V., 1897, pp. 91-104.

ress on vision and none on hearing, and conversely. For instruction it is not desirable to drive a flock of twenty students successively into different rooms. They should use the available space of the laboratory, apparatus set up permanently, as an optical bench or chronograph, where it stands, movable pieces, as a perimeter or tuning-forks, where there are convenient places. A room for taste and smell can scarcely be used continuously for research, and a special room is not needed for an hour's instruction in the course of a year. Doubtless Professor Titchener does not hesitate to violate the names of his rooms and use them to the best advantage. I only make these remarks for those who may plan a laboratory on the Cornell model.

Professor French, in claiming that experimental psychology has no place in the undergraduate course, appears to me to fall into the common error of regarding experimental psychology as a science apart from psychology. It is, I hold, simply a method in psychology, and I do not see how an adequate course in general psychology can be given by any one who ignores this method. Recent text-books—James, Ladd, Külpe, Jodl, Titchener, Ebbinghaus and others—certainly show that the results of the experimental method are an integral part of the science of psychology.¹ If students will follow only one course in psychology they would naturally be given a general course. If the teacher has independent interests these would be emphasized, but I should regard the subjects of James's *Principles* as a safe guide. This course should be given for Sophomores and Juniors, and subsequently a university should be able to offer courses in physiological, experimental, genetic, analytical, philosophical and historical psychology.² The special student of psychology should follow all these courses, partly before and partly after taking his B.A.; other students should elect in accordance with their interests and intentions.

I am glad that Professor Titchener advocates laboratory work as an elective course for undergraduates. Still I am not quite so enthusiastic about this as I used to be. An eminent professor of physics

¹ When Dr. Harris writes, in his *Psychologic Foundations of Education*, that from the 'psycho-physiological laboratories' no 'results in pure psychology of a positive character will be arrived at,' the chief information conveyed is that Dr. Harris is concerned with a psychology that belongs to the past.

² Whether a special course in experimental psychology should be offered by a college as an elective seems to depend on the resources of the college. Vassar, with four or five hundred students, many of whom intend to become teachers, should, I think, provide such a course.

recently told me that he did not want to turn his students into the laboratory until they had followed a four-hour course of lectures through the year. In psychology, where the experiments are in a way less fundamental and have scarcely as yet been perfected by the survival of the fit, this point of view is even more tenable. Perhaps an independent lecture course should be offered, and a course of supplementary laboratory work which could be elected simultaneously or the following year.

In a course of lectures on experimental psychology, demonstrations can with much economy to the teacher and student precede, and in some cases replace, laboratory work. Dr. Scripture's lantern seems to perform a useful service; the mere darkening of the room has a pedagogic function in concentrating attention. With the lantern it is possible not only to show pictures, but also to demonstrate many of the phenomena of vision, to exhibit words, etc., for association and memory, and to carry out actual experiments on fatigue, reaction-time, etc. The psycho-physical methods can be applied to sight, hearing and the perception of space nearly as well by a whole class as by the individual student. Dr. Ward, in his course on 'Psychophysics' at Cambridge, used to devote hours to explaining the psycho-physical methods, and could, I think, have saved half the time by a demonstration. It follows from the above that I think Professor Titchener mistaken in not providing for class experiments.

Some demonstrations seem to me also desirable in a general psychological course. The student has his text-book and the lecturer may not be able to add very much, but illustrations may be given, which will greatly increase the interest of the course and be a real economy for the student. If subjects such as the senses, the accuracy of discrimination, the perception of space, illusions of sense, the elements of music, memory, the association of ideas, etc., are treated—and they must be treated—a demonstration or a class experiment will add as much as in physical or chemical lectures. During the years I spent at Leipzig Professor Wundt added many demonstrations to his general course, and I think to its advantage.

Dr. Scripture, from his abundant experience, is able to make many useful suggestions regarding the conduct of a psychological laboratory. Possibly he over-emphasizes the laboratory as compared with psychology. The student who intends to undertake research should understand methods of measurement and the adjustment of observations, but perhaps he should be referred to the physical laboratory. Cer-

tainly it is not a necessary function of a psychological laboratory to teach the use of the lathe. If the student can work in metals he has an advantage, though he is also in danger of wasting time. The calculus may ultimately become important in psychology, but as yet no one has accomplished much by its application. Dr. Scripture's ideas of profit and loss are rather material. Whether the use of an instrument costs eight cents or three cents per hour is not as important as he seems to think. When he writes: "The best laboratory * * * is the one that yields the largest net results in scientific research and instruction for each dollar expended," I should only agree on condition that the last four words be omitted. The question of an expensive outfit has two sides. On the one hand, the best research can be done, and has usually been done, with simple means. On the other hand, it is bad economy for a university to spend thousands of dollars on buildings and salaries and reduce the efficiency of the laboratory to one-half by not providing a workshop and adequate instruments.

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PSYCHOLOGICAL LITERATURE.

La psychologie à l'école primaire. A. BINET et N. VASCHIDE.
L'Année Psychologique, 4me Année. 1898. Pp. 1-14.

This, the opening title of the fourth volume of the *Année*, may be taken as a general title covering the larger part of its original contributions. Of the twenty-five chapters of Part I., pp. 1-431, which contains the original memoirs, the first seven, pp. 1-172, describe a series of tests upon school children; the next five, pp. 173-244, a similar series of control tests in the normal school, while the following six chapters, pp. 245-315, present supplementary studies growing out of these tests—all by the same authors and forming practically one article.

M. Binet tells us that during the ten years since he gave up abnormal psychology by far the larger part of his experiments have been made in the schools. But from this and other recent papers it is evident that he has now turned his attention much more seriously in this direction. In the introductory chapter he gives an entertaining account of the different classes of people among whom he has tried to carry on psychological experiments and of the trouble he has had in finding volunteers willing to undergo the inconvenience incident to extended researches. In the schools and colleges, on the other hand, with their large numbers under uniform conditions and subject to authority, he sees the promise of unlimited material for the psychologist.

Nothing is said of the pedagogical bearing of the studies here presented. This may be due to the preliminary character of the work, or to the fact that researches with educational aims in view are to appear in the *Bibliothèque de Pédagogie et de Psychologie*. Indeed, the authors are not overbold in claiming psychological results of great value. They lay emphasis rather upon the opening of new problems and the suggesting of new methods. In many of the chapters the work is spoken of as preliminary and the results as tentative. In view of the moderate claims of the authors one is not disposed to regard the smallness of the results in a critical spirit, but rather to be thankful for numerous suggestions and await with interest future reports.

It is certainly a hopeful sign for those who have faith in the peda-

gological possibilities of experimental psychology that practically all the energy of the Paris laboratory is for the time being devoted to child-study. The number of workers in this field is growing. To be sure, they have spent most of their time thus far in discussing problems and methods, but one is confident that they are working in a field of great promise, and that results are sure to be forthcoming of value both to the teacher and to the psychologist.

The work here described extended through the spring of 1897. The tests were first made upon forty boys of an average age of thirteen, in the upper classes of one of the Paris public schools. Afterwards they were repeated for the sake of comparison upon forty young men, eighteen to twenty years old, in the Versailles normal school. Thanks to previous practice, the control tests were completed in eight days. There were four sets of measurements, including tests of muscle force, tests of quickness, studies in respiration and circulation, and anatomical measurements. In all cases the tables give us the maximum, minimum and average records, also the average of the class divided into four groups according to their records in each particular test. The mean variation is not used. Several photographs show clearly the arrangement of the apparatus and at the same time give us a glimpse of the genial faces of M. Binet and his colleague.

The tests of muscle force, Chapters II. and VIII., which form by far the most important part of the work, are reviewed below. The tests in quickness, Chapters III. and IX., included reaction time, choice time, rate of tapping, quickness of pressure on the dynamometer, and running. The numerical results in the reaction-time tests are of comparatively little absolute value. For instance, the average time of the forty adults, 186.2 σ , is longer than that of the forty children, 166.8 σ . This difference from the usual result is supposed to be due to an error in controlling the chronoscope. The d'Arsonval chronoscope was used and records are given in hundredths of a second. In several instances an attempt is made to discover differences on the basis of insufficient data. An extreme case is an attempt to show the influence of a remark made to the subject in the middle of a series of reactions. The table shows that in one trial, with five persons out of seven the reaction following the remark was from two to seven hundredths of a second longer than the one before. Nothing is said about the other figures in the series. It may be remarked in passing that it is rather unusual that five out of the seven should have a normal reaction-time of 100 σ or less.

But if figures are at a discount the careful study of the physical

signs accompanying the reaction is suggestive. Three types of reaction are distinguished: The diffuse type, involving a large number of muscles, as distinguished from the localized type, where the movement is confined to the fingers; second, the semi-voluntary, marked by anticipatory movements of the hand; third, the emotive type, characterized by trembling of the hands. The most important sign of expectant attention was found to be a fibrillar trembling of the pupil of the eye.

In choice time the subject reacted to one of two sounds and not to the other. As a rule, reactions to the wrong sound decreased from the beginning to the end of the series, but individuals varied greatly in the rapidity with which they thus adapted themselves to new mental conditions. The fact that the average time of reacting to the wrong signal, 278.5 σ , is nearly equal to that required to react to the right signal, 289.8 σ , is suggested as a criterion to determine whether a subject is really making choice reactions.

In the third test of quickness the rate of tapping was determined by the number of dots which the subject could make with a pencil in five seconds. The average for the boys was 32.52, for the adults 35.11. The rate increased in each of four trials, the slower subjects making the greatest gain. In the fourth test the time was measured which was required to run a certain distance, 30 meters for the boys, 200 and 400 meters for the adults. In the third test the Ch. Henri dynamometer was used to measure in tenths of a second the time between the beginning of a pressure and its maximum. The figures are 1.438 seconds for the left hand and 1.623 seconds for the right, the left hand always being quicker than the right.

It is surprising that a comparison between the results of the different tests for quickness shows no relation between them, except to a slight degree in the case of simple reaction-time and the rate of tapping.

In the tests on respiration and circulation, Chapters IV. and X., we have the lung capacity measured by the Dupont spirometer, the chest measure, normal and expanded, and the distance at which the subjects could blow out a candle. The results in the candle test disagree and prove this method worthless. Two points come out clearly in the study of the pulse. First, a slight emotion of fear and attention produces an acceleration of the pulse. The average increase for the boys as they came into the room for the first experiment was 18 beats per minute. Second, intense, concentrated muscular work decreases the pulse rate. The loss after the dynamometer test averaged

17, after the rope climbing 30 beats per minute. General exercise, such as walking and running, increases the rate.

Chapters V. and X. give the averages of the anatomical measurements suggested by M. Manouvrier: weight, height, length of trunk, girth of shoulders, arm, wrist, length of limb, arm, five head diameters, length of medius, length of stride.

The next chapters, VI. and XI., discuss the index of variability of the results. This is determined by dividing the subjects into four groups according to their rank in a test and taking the percental relation between the first group and the last. The results show that the variability is least for the dimensions of the head and increases in the following order: height, trunk, weight, lung capacity, vertical traction, strength of left hand, strength of right hand, reaction-time. Fatigue increases the difference between individuals.

The final chapters in the treatment of these results, VII., XII., seek to correlate the results of all the tests. The names of the subjects are divided into four groups according to their rank in one test, and opposite each group is placed the average which this group obtained in each of the other tests. The first results show a surprising uniformity with which the four groups maintain the same relative standing in all the tests. In the control experiments this is not true to the same extent. In both cases it is worthy of note that it is the group with average memory for figures, and not the group with the best memory, which is physically superior.

In the second method of correlation the names are arranged according to their rank in one test, and opposite each name is placed the rank of this person in each of the other tests. The sum-total of these ranks gives a composite figure which shows, in a general way, the relative standing of the subjects with reference to the tests as a whole. Perhaps the most interesting page of all is a series of photographs of the forty boys arranged according to this general record. Many questions suggest themselves as one tries to penetrate the gradual change in their interesting faces.

CHARLES B. BLISS.

Expériences de force musculaire et de fond chez les jeunes garçons.

A. BINET et N. VASCHIDE. L'Année psychologique, Vol. IV.
pp. 15-63.

La mesure de la force musculaire chez les jeunes gens. La force de pression de la main, la traction, la corde lisse, le saut. Ibid.,
pp. 173-199.

Corrélation des tests de force physique. Ibid., pp. 236-244.

Critique du dynamomètre ordinaire. Ibid., pp. 245-252.

Examen critique de l'ergographe de Mosso. Ibid., pp. 253-266.

Réparation de la fatigue musculaire. Ibid., pp. 295-302.

Un nouvel ergographe, dit ergographe à ressort. Ibid., pp. 303-315.

The first three articles noted above give results of a part of a larger series of anthropometric experiments and tests on forty-three boys of an elementary school (*jeunes garçons*) and on about an equal number of normal school students (*jeunes gens*). The strength tests included the usual test of grip with oval dynamometer, right and left hands, vertical pull for strength of back, climbing a rope for strength of arms, long and high jumps to test legs (used only for the older pupils), and two tests of endurance using respectively a Mosso ergograph and the oval dynamometer. It should be noted that most of the experiments were made while the pupils were stimulated by emulation, and the results thus obtained are to some extent abnormal.

The forty-three boys showed an average strength of grip 20.96 kg. right hand, 16.5 left, while the results of the normal school youths were 40.6 and 36.8, respectively. Later in the article the authors show that all this difference may not be due to the age and strength of the subjects. Further experiments seem to prove that the relative size of the dynamometer and of the hand is an important factor, an average for eight pupils being 20.5 kg. with an adult's dynamometer (5 cm. diameter), while a small instrument (3.8 cm. diameter) under similar conditions showed for the same subjects only 16.6 kg. Thus, when the hands are shorter and the dynamometer is squeezed with the third phalanx instead of the second, one finds a decided decrease—average for three subjects, using second phalanx, 28; with third, 15 kg.

Strength of back and legs was tested by pulling vertically on an oval spring, the results for boys and youths being respectively—average, 77 and 146.6; maximum pull, 121 and 187; minimum, 56 and 102.

Climbing a rope was advised by two instructors of gymnastics as an excellent test of general physical ability of the individual. Eight boys mounted the total length of the rope, 1.9 meters; five went half way; five ascended one-quarter the distance, while twelve could not advance beyond the first position. The results for the more advanced pupils are: total length, seventeen; three-quarters, three; one-half, twelve; one-quarter, two; none, three. The test as a measure of the muscular ability is thus seen to be a poor one, because it does not

measure the force of these weak children who are not able to raise themselves by their arms, and to group all the weak ones seems questionable since they are not all equally weak. As a differential test, however, it may be good, sharply marking off, as it does, the weak from the strong.

A similar criticism may be made of the high-jump test, wherein fourteen jumped a height of 1.7 m., eight jumped 1.5 m., while fourteen could not go over the established lower limit. The average running jump was 4 meters; maximum, 5.25; minimum, 3. The authors point out the analogy between these tests and the oval dynamometer, but no mention is made of the effect of practice, a factor more likely to enter here in the case of boys than in the grip experiment.

Five successive experiments with the dynamometer were used to give some indication of the endurance of the younger children, with following average results:

Right hand—23.9, 22.5, 22.7, 21.8, 22.2.

Left hand—21.2, 20.7, 20.5, 20.6, 19.8.

These experiments were made when the children were incited by emulation, both by direct appeal of the teacher and by the presence of their fellows. The results of a similar set without emulation follow:

Right hand—21.5, 19.4, 19.7, 19.2, 18.6.

Left hand—18.3, 17.6, 17.1, 16.9, 16.6.

The average results of a corresponding series of ten experiments with emulation upon the normal school students are as follows:

Right hand—40.6, 40.8, 41.1, 40.5, 40.0, 39.4, 39.2, 38.8, 38.6, 40.1.

Left hand—36.8, 36.3, 37.0, 36.5, 35.3, 34.3, 35.1, 34.8, 35.1, 35.3.

Another test of endurance was made using Mosso's ergograph, but, unfortunately, the results from the two classes are not comparable, two kg. having been used for the younger and five for the older scholars. However, the individual differences are considerable and the results are very interesting. The primary boys averaged 35.5 pulls before becoming thoroughly fatigued; maximum, 102; minimum, 0; while the normal scholars averaged 38; maximum, 60; minimum, 24. The average maximum height to which the latter subjects raised the 5 kg. was 18 mm., and the total average height 15 mm. The authors have used this method of characterizing differences instead of the physical work in kilogrammeters, since all the factors noted above may vary without altering the amount of work done, and each must be considered separately to give the indications of endurance.

Besides these actual results of the experiments the authors have attempted to correlate the results of the different tests, a subject which, they say, is yet only 'a first attempt,' interest in which consists chiefly in their having '*posé une question nouvelle*' (*sic*). Two methods were adopted by the writers. The first consisted of dividing the subjects of any given test into four classes—strongest, medium strong, medium weak, weakest—and arranging the results in the other experiments for the same classes. The averages obtained were then directly compared with the results of the test chosen. In the second method the subjects were ranked in a given test from strongest to weakest, and the rank of each was then found in any other test, the figures averaged for the four classes and comparison made; or the individual differences in rank were noted and arranged for the four classes. In general, a direct relation was found to exist between the different strength tests.

In another article MM. Binet and Vaschide give results of experiments upon the time necessary for recovery from muscular fatigue. The writers used the ergograph of Mosso, repeating some of Maggiora's experiments in this line, but allowing different times for recovery. Thirty minutes were found to rest thoroughly the muscles employed, while after an interval of two and one-half minutes' rest a decided decline from the normal was noted. A third trial after another two and one-half minutes gave only a slight additional decrease. Over twenty students were thus tested, and the tables show some striking individual differences, but the authors do not discuss this aspect of the problem.

Throughout the articles no account of the variations from any of the averages is given. The writers seem not to understand the value of this. Any account of correlation without the average variation, probable error or other similar figure is almost valueless, as we must depend upon it in making our judgment of the probability of the correlation. This error of omission is, unfortunately, not the greatest to be found in the articles. Slight mistakes in calculation are, alas! too numerous (*e. g.*, see page 26, under 2. *Type stationnaire, Main gauche*, where each of the five averages is wrongly calculated. The fourth average under *Main droite* should be 18.4 instead of 19.4. See also page 27, where under 3. *Type de la décroissance continue, Main gauche*, the fifth average should be 17.28 instead of 15.06. The last two mistakes cannot be considered typographical errors, as they are reproduced in the curves, pp. 24 and 25). Typographical errors also are not infrequent, and the whole series of articles shows marks of haste.

In three articles the same authors criticise the ordinary oval dynamometer and Mosso's ergograph and describe a new spring ergograph devised by themselves. The oval dynamometer, they say, is cheap and easily handled, but time is necessary for reading it, and a rapid series is impossible. The muscles used are not known; in a long series different muscles may be used, while a slight change in position causes a great difference in apparent power. It is liable to slip; it pains the hands considerably; and it does not register the mechanical work done. Mosso attempted to overcome some of these difficulties and constructed his ergograph using a weight instead of a spring. With this apparatus one can calculate the mechanical work done and the muscle or set of muscles can be well isolated. However, as a mechanical instrument the ergograph is not without its fault—the weight hangs over a pulley at a right angle to the direction of the pull and the position of the finger during flexion changes, so that the pull is almost never straight on the carriage, but at a varying angle. Moreover, at first one does not always make a pull of maximum force with the weight given, and the conditions throughout the experiment are not constant. Finally, when one cannot raise five or three kg. one may still be able to do some work, say one kg. This lesser ability the ergograph does not record, and the mechanical work cannot, therefore, be used as a measure of the physiological. To overcome these difficulties the writers had constructed a new ergograph. This consists mainly of a spring with attachment for finger and a pen for recording the movements on a moving surface. Its advantages are that the subject can from first to last give all his force to the work and the least effort is recorded. Each subject is the judge of his own total ability, and no arbitrary standard, one, two or five kg. is set. The finger attachment is ingeniously devised, and this alone will do much to make results more constant. A comparison of experimental results from this instrument and from Mosso's seems to show some of the former's advantages. It should be remarked that Professor Cattell, with the faults of Mosso's instrument in view, had had constructed and exhibited before the American Physiological Association (May, 1897) a spring ergometer differing, however, in many ways from that of MM. Binet and Vaschide (see *Science*, N. S., Vol. V., p. 909). Both instruments seem to be a decided advance over that of the Italian physiologist, and one must regret that so much of the work recorded in the present *Année* was done with the older instrument when the writers had in view its many disadvantages.

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La consommation du pain pendant une année scolaire. By A. BINET. *L'Année psychologique*, 1898, IV., pp. 337-355.

Professor Binet indicates a comparatively easy method of investigating the relation between mental work and physical exhaustion. By arrangement with the authorities of four normal schools (two for males and two for females) and one house of detention, a record was kept of the daily consumption of bread in each institution, as well as the number fed each day. The author argues that since the daily allowance of other aliments remains fixed throughout the year in these institutions, while the supply of bread is not limited, the latter will serve to satisfy the demand for additional nutriment; hence, the fluctuations in the consumption of bread will indicate the changes in the needs of the subjects from time to time.

To eliminate daily fluctuations, the average daily consumption for each month was determined. Comparing the figures for each month, in the institutions separately, the author found among all the schools a steady diminution in the daily average as the school year advanced, with the exception of a single marked advance, which occurred immediately after the spring vacation. In the prison record, on the other hand, the consumption showed a general tendency to increase during the comparable months (excepting January and February), though with certain notable fluctuations, for whose explanation data were lacking. Comparison between the two classes of records left no doubt that the decreased consumption in the schools was due to the steady continuance of intellectual labor. A special analysis of the daily consumption at the normal school of Versailles in examination time showed a slight increase during the days of written examinations, and a more marked decrease during the days of oral examinations.

The results show, according to the author, that "prolonged intellectual work may give rise to a weakness which manifests itself not only in the psychic functions but also in the functions of the organic life." From the hygienic point of view, he thinks, it is still an open question whether or not this weakening of the functions is necessarily injurious. Aside from the apparent laxity of such an investigation in certain respects, and apart from the interpretation of the results, which should be submitted to a rigorous analysis, this study is a real addition to the methodology of psycho-physical research.

Description d'un objet. By A. LECLÈRE. *L'Année psychologique* 1898, IV., pp. 379-389.

The experiment consisted in showing a watch to a class of 30

girls, between 13 and 17 years of age, and asking them to write down whatever was suggested by the object. Having examined the results, the author found 7 principal kinds of statement, corresponding to as many different types of mental activity. These he names, respectively, description, observation, imagination, moral reflection, erudition, emotion (pure and simple) and æsthetic emotion.

No one of the papers contained all seven; at most four were found in a single paper (in five cases). Ten papers were principally description, five principally observation, five principally moral reflection. Altogether, description appeared in fifteen, observation in twelve and moral reflection in seventeen, with traces in others; (this large proportion was probably due to the fact that the experiment was conducted by the professor of moral philosophy). Emotion, æsthetic emotion and imagination were found in few instances; erudition was practically wanting.

The author believes that the papers furnish a fair measure of the state of mental development at that age, which may be compared with types of mind more or less advanced, by means of similar experiments. The measure is found in the set, however, rather than in the single papers; for a certain class of statements may be omitted from a paper, not because the writer lacks that type, but because only one mental type can be realized at a time, and subjects of that age have not sufficient mental breadth to pass readily from one type to another.

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Psychology and Education. By HUGO MÜNSTERBERG. Educational Review, Sept., 1898.

In this article Professor Münsterberg undertakes to make a non-partisan analysis of the manifold relations that exist between psychology, especially that of childhood, on the one hand, and the practice of teaching on the other. The earlier part of the article is devoted to an examination of the bearing that psychology may have upon childhood. Thus, (1) the child (defined as a person below the age of twenty-five) may be taught psychology; or (2) may be taught according to a scheme of psychology; or (3) may be the object of psychology; or (4) may be an instrument for the study of psychology, etc. If the child is to be taught psychology this may be done (1) at different ages, (2) by different methods, (3) by presenting various parts of the subject. Incidentally the opinion is expressed that the high school period is not too early to begin the study, but the instruction must be

given by competent teachers. The metaphysical, pathological and anatomical aspects of the subject should be avoided.

Concerning the child's mind as an object of psychological study, Professor Münsterberg shows first that in such a study the mind of the child may be treated as a thing apart, the investigation being pursued as an end in itself. For such a view he has unqualified condemnation, saying that a botany of blossoms would be as rational as a psychology of childhood. The human being is a whole, who has indeed a development, but not discrete states of existence. Psychology deals with the *facts* of consciousness and the *laws* that explain them; it will look in vain for unique facts or laws in the child. To study the child's mind as a part of general psychology is perfectly legitimate, since this is merely to study mind genetically. The pathological study of the child is for the physician, not the teacher. In thus making the child's mind an object of psychological attention we may take either of two courses: (1) make use of the whole of psychology in this study of the child, or (2) make the study of children a new standpoint for the whole field of psychology—a new path of entrance.

The second and educationally important part of the paper discusses the question whether the teacher is directly or only indirectly assisted by psychology. The issue is clearly presented in the following quotation: "Is psychology valuable to the teacher for his teaching methods directly, or only indirectly through the medium of a scientific educational theory? In the first case the teacher himself transforms his psychological knowledge into educational activity; in the other case educational theory has performed for him the crystallization of educational principles out of psychological substances, and he can follow its advice, perhaps, even without knowing himself anything about psychology."

Having thus clearly stated the issue, Professor Münsterberg enters upon a vigorous argument to show the worthlessness of what one might call the 'cook-book' application of psychological theory to educational practice; this holds equally of 'child study,' 'experimental' and 'physiological' psychology. One cannot expect to find educational maxims in detached portions of psychology; the educational processes would not be changed were the nervous centers in the liver rather than in the brain. Plato's theory of the distribution of the seats of psychical powers would serve the teacher just as well as modern brain anatomy.

If psychology refuses to furnish educational receipts how can it be of service to education? Only through the mediating channel of sci-

entific educational theory. The educational experts of this generation should frame a scientific theory of education in which psychology, so far as it pertains to the education of the young, will find its due embodiment. "Do we not lay a special linking science everywhere else between the theory and the practical work? We have engineering between physics and the practical workingman in the mills; we have a scientific medicine between the natural sciences and the physician." At this point Professor Münsterberg takes rather a low view of the teacher, since it is intimated that if he only follows the theory he may get along without a knowledge of the underlying sciences. But the quack is the man who follows medical theory without understanding chemistry, physiology and the like; the mere mechanic is one who has a rule-of-thumb knowledge of engineering, but who knows neither mathematics nor physics; would not a similar reproach attach to the teacher who followed blindly any theory of education, however good?

Professor Münsterberg very justly recognizes the fact that educational theory is not founded alone on psychology, but also on ethics, æsthetics and sociology, on history and religion. He also urges that, so far as psychology is concerned, not all, but only special parts are valuable to education. The proper place for the selection and investigation of psychological problems valuable to education is the psycho-educational laboratory, located presumably where the leaders of education are trained—in the university.

It will now be seen that Professor Münsterberg's main contention is that psychology of whatever variety is valueless to the teacher, except through the clarifying medium of educational theory. One may, perhaps, dissent from so sweeping an assertion without greatly diminishing one's sense of the truth and importance of the contribution as a whole. Professor Münsterberg maintains that, whereas a study of the child's environment may enable teachers to understand him, psychology will merely enable them to analyze him. Is not this a strange doctrine for a psychologist? Is it the mission of this science ever to analyze, never to understand? What becomes of the definition with which the article opens? It is there declared that the function of psychology is to find the *facts* of mental life, perhaps by analysis, and then to explain them through the discovery of the *laws* that govern them. But is not the very kernel of *understanding* a grasp of the laws that underlie phenomena? Surely, then, psychology analyzes only that it may understand. If this is true there appears to be no *a priori* reason why psychological study of children will not assist a teacher to understand as well as to analyze them.

As a matter of fact, most teachers begin their work with but a mechanical comprehension of educational theory; its mastery is the result of many years of study of the underlying sciences, of which psychology is one of the most important. Renewed study brings fresh insight into the intelligence, disposition, interests and efficiency of youth. It would be no greater error to affirm that the chief need of all the half-educated teachers of the United States is a course in experimental psychology, than to make the equally extravagant statement that psychology of whatever kind is valueless to the teacher directly; for it helps him to understand the nature and amount of the pupil's self-activity; to distinguish his original from his ideal nature; to comprehend the amount and kind of thinking ideally involved in the study of a given subject, like physics or chemistry; to apprehend the pupil's rights and duties as a member of a social, a civil, an ethical or a religious community; and, finally, it helps to make rational methods of teaching more comprehensible, hence more prevalent.

In spite of this dissent to what may be called the pet doctrine of the article, Professor Münsterberg's contribution is likely to be generally recognized as one of the most important yet made to clear thinking where the relations of psychology to education are involved.

CHARLES DEGARMO.

CORNELL UNIVERSITY.

The Story of the Mind. JAMES MARK BALDWIN. The Library of Useful Stories. New York, Appleton. 1898. Pp. 232. 40 cts.

In this little work, Professor Baldwin has attempted, like Socrates of Athens, to bring philosophy down from the heavens to earth and to the homes of men. Indeed, the making of *primers* of psychology is now quite the mode. It is a very good sign, indeed, when a science reaches the stage where it can be written in the form of a primer, and it is a real test of a scientist's ability and acquaintance with his subject when he can write a primer—particularly a primer of psychology. So one opens a book of this kind with a certain fear and dread. The work before us, however, speedily allays such apprehension. It is a useful summary of psychological methods and results. It turns out, indeed, to be not really a primer of psychology at all, but rather an elementary introduction to psychological methods. We miss wholly, but not inconsolably, the usual routine of the elements of psychology, and the whole subject is with a wide sweep 'rounded up,' as it were, into a brief compass. In a series of chapters with such headings as 'The Mind of the Animal,' 'The Mind of the Child,' and 'The Con-

nection of Body and Mind,' it treats of comparative, physiological, introspective, experimental, abnormal, educational and social psychology, and the psychology of the child and the genius. Although it has little to say about sensation, perception, and memory images, it gives one a vivid conception of what a big thing the mind really is and what a place it has in nature, in history, in society. The style is simple and modest. Controversies are, of course, avoided. Two of the best chapters are on 'The Mind of the Child' and 'The Training of the Mind.' They contain good psychology and wise and useful pedagogy, and illustrate, as does the whole book, the author's independence and originality.

It would be as easy as it would be graceless to criticise a book of so wide a plan and so narrow a compass. The preface prepares readers of Professor Baldwin's other works to recognize certain old friends, the recognition extending in some cases through whole chapters, as in the psychology of the *socius* and the genius; but this is hardly a fault, as these parts represent some of the author's original contributions to the science of psychology, and make the book something more than a mere dry summary of methods and facts. In several chapters, however, it would perhaps have been better in telling the story of the mind to have summarized more and selected less. The subjects of animal psychology, physiological psychology and experimental psychology are treated merely by a few selected topics quite inadequate to give the beginner a faithful conception of these methods. A similar fault is observed in the excellent chapter on the mind of the child. It discusses very reasonably the methods and limitations of child study and traces very acutely the early development of the child's mind, but it omits to mention a large group of studies upon children of the school age, which, while containing some abuses and suffering all kinds of criticism, have nevertheless created a new science and promise in the end to produce results no less than revolutionary in their influence upon practical education. To my mind the frequent 'warnings' to teachers against child study, warnings which even Professor Baldwin seems to find needful, are exceedingly untimely. For the love of heaven and a rational education, let them do what they will! It is little enough. As well warn a boy against learning to write, because at first he writes with his tongue and his eyes and his head and both arms, as to warn teachers against child study. It should be said, however, that Professor Baldwin's general position is one of warm sympathy towards child study in the forms which he finds legitimate, and it is with great pleasure that we notice that the longest chapter in this little book is the one upon the mind of the child.

G. T. W. PATRICK.

A Method of Recording Eye Movements. By E. B. DELABARRE.

Am. Jour. Psych., Vol. IX., No. 4, pp. 572-4, July, 1898.

Preliminary Experiments in the Physiology and Psychology of Reading. By E. B. HUEY. *Id.*, pp. 575-86.

Professor Delabarre finds that a shell or cap made of plaster of Paris and fitted over the cornea of the cocained eye will retain its position until soaked off, and may, when connected mechanically with a kymograph, be used to register and record the lateral and vertical movements of the eye. He connects this eye-cap with the kymograph cylinder by means of a thread and a pulley. Huey, following him, used somewhat the same eye-cap with a lever acting directly upon the revolving drum, which recorded the quick lateral motions of the eye, better than Delabarre's instrument did. Neither finds any serious injury to the eyeball resulting from the use of plaster of Paris. (The use of holocaine would obviate any possible effect upon accommodation due to cocaine.) Two objections may be made to the use of plaster of Paris. The eye-cap must be made thick enough not to break, its weight being, therefore, a perturbing, even though constant, factor, and, furthermore, the eyelids have to be kept from contact with the plaster of Paris. Professor Delabarre holds them apart with 'a wide opening spring with tortoise-shell grooves for the lids' and Huey used 'lead fingers fastened to the head frame and pressing sufficiently upon the skin above and below the eye.' In Huey's experiment III. the use of this apparatus, in reading lines of different lengths, showed that the eye does not move from end to end of the line, but turns at some distance from either end. (Delabarre notices that the point of fixation of the eye is not always the one intended, and thinks that in fixating an arrow-head the actual point of fixation falls within the angle.) Huey observes finally that "the lateral movement decreases much faster than does the length of the lines and that at 21 mm. the reading may be done without lateral movement."

Huey's experiments were largely speed tests upon reading matter printed horizontally and vertically, *i. e.*, one word printed under the other. In his first experiment (four subjects) he used fifty letters, then fifty words of two letters, then the same number of three-letter words, and so on, up to words of sixteen letters, all in nonsense order. Each series was printed horizontally and vertically, and was read through twice at greatest speed. Words of five and eight letters, and all of eleven letters and more, were read more quickly when printed in the vertical position, all the others being read better when in ordinary horizontal position. This experiment, therefore, affords no ground

for changing the present order of printing, as the words of the English language average only five letters each. Huey makes no mention of the syllabification of his test words, which is an important element in experiments where they are read aloud. The criticism may also be made that a cacophonous juxtaposition of words (like "Irks care the crop-full bird? Frets doubt the maw-crammed beast?") would decidedly retard the rate of articulation. He does not say whether his subjects thought they were hindered in this way or not. Words even in nonsense arrangement may be placed euphoniously. In another experiment Huey's three subjects 'read aloud, as fast as possible, a sense passage of 300 words divided into six parts of fifty words each.' In this experiment the average number of words read per second was 4.36 horizontally and 3.93 vertically. The same passage was read silently by the same subjects at the rate of 7.54 per second horizontally and 4.83 vertically. In a third experiment the importance of the first and last parts of words, for recognition, was tested. A passage was printed showing (*a*) the first half and (*b*) the second half of every word (chopping letters in two in words with an odd number of letters). In this experiment .33 words per second were correctly read when the second half of every word was shown, and .49 per second when the first half was shown. It seems to the present writer that better results would follow the study of the part of the word fixated by the eye in ordinary reading, as recorded by the instrument described by Mr. Huey.

WILFRID LAY.

Die Aehnlichkeitsassociation. KARL DEFFNER. Zeitschrift für Psychologie u. Physiologie der Sinnesorganen, Bd. XVIII., 3. 9 Aug., 1898.

German reviewers are so prompt in their criticism of psychological essays which fail to refer copiously to German psychological literature that one may be pardoned for uttering a sort of warning to the would-be reader of this paper on similarity association, which makes no allusion to English psychology, and which brings forward a theory substantially identical with Herbert Spencer's without an allusion either to Spencer himself or to the trenchant criticism of Bradley and of James upon his theory. Deffner announces in the beginning his agreement with Lipps, to whose lectures frequent reference is made, as the starting-point of the present work. The avowed intention of the monograph is to restore similarity association to its rights. This is accomplished by definitely abandoning the simple position of ana-

lytic psychology in accordance with which association is 'the way in which, in our consciousness, psychic contents follow upon each other,' and by assuming that association unites unconscious, as well as conscious, 'psychic events,' and that it is 'Cause * * * which brings about the psychic movement,' 'psychic *Potenz* or disposition,' or, as later stated (p. 224), a 'tendency of the Psyche's progress.' The only vindication of this metaphysical treatment of a psychological subject is the remark (p. 219) that psychology must take refuge in this hypothesis or else abandon the hope of 'understanding' association. But better respectable ignorance than recourse to such violent postulates!

The paper which follows upon this unpromising introduction is a curious and tedious compound of superficial description and classification, of misleading analogy, of obvious illustration and of ineffective criticism (mainly directed against Külpe). A long and occasionally suggestive analysis of musical experience discloses the associativeness of what are indiscriminately called 'relations' (*Beziehungen*), which turn out to be musical rhythm, intensities of light and sound or emotional moods, as occasion demands. But from the simple, scientific treatment of these 'relations' as themselves contents of consciousness Deffner, of course, carefully guards himself, assuming, without any justification for his view, that 'contents' are necessarily sensational.

It is only fair to add that Deffner does briefly refer to what is, in the opinion of the writer, the correct classification of association, the reduction of 'similarity' to 'contiguity' association, by treating similarity as the persistence of an identical content, concrete or simple, which then becomes the starting-point of contiguity association, so that only the dissimilar features of the suggested fact of consciousness are really 'associated.' To this Deffner opposes (p. 232) only the old argument (which, even in Höffding's able statement of it,¹ can not stand out against the appeal to experience) that only concretes can thus 'persist in consciousness,' and that for this reason subtle and delicate similarities of quality or tone are unaccounted for by the theory. More positively he argues that similarity association is at the basis of every case of contiguity association, since my 'present image,' if it is to associate a 'past percept,' must at first call up the 'past percept,' which it exactly resembles. This is the Spencer and Höffding theory of the fundamental sort of similarity association underlying all recognition, and its fatal weakness has repeatedly been exposed—by James,

¹ *Vierteljahrschrift f. Wiss. Philos.*, XIV., 168 seq.

by Bradley, by Lehmann and by many others—the impossible Herbartian assumption of revivable past states of consciousness.

The conclusion forced upon the reader of this monograph is, therefore, the reflection that the theories which it defends are better formulated by other writers.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

NEW BOOKS.

Elements of Sociology. F. H. GIDDINGS. New York and London, The Macmillan Company. 1898. Pp. xi+353. \$1.10.

Problems in Philosophy. JOHN GRIER HIBBEN. New York, Charles Scribner's Sons. 1898. Pp. vi+203.

An Introductory Logic. JAMES EDWARD CREIGHTON. New York and London, The Macmillan Company. 1898. Pp. xiv+392. \$1.10.

Human Immortality; Two Supposed Objections to the Doctrine. WILLIAM JAMES. Boston and New York, Houghton, Mifflin & Co. 1898. Pp. 70. \$1.00.

Theories of the Will in the History of Philosophy. ARCH. ALEXANDER. New York, Scribners. 1898. Pp. x+357.

The Play of Animals. K. GROOS. Trans. by E. L. BALDWIN with Preface by J. MARK BALDWIN. New York, Appletons; London, Chapman & Hall. 1898. Pp. 23+341.

The Story of the Mind. J. MARK BALDWIN. Useful Story Series, New York, Appletons; London, Newnes. 1898. Pp. ix+236, 12mo. 40 cents.

Études de psychologie sociale. G. TARDE. Tome XIV. of Bib. Soc. Int. ^{III}. Paris, Giard et Brière. 1898. Pp. 326. Broché. 7 fr.

Pensées de Tolstoi d'après les textes Russes. OSSIP-LOURIE. Paris, Alcan. 1898. Pp. xx+179. Fr. 2.50.

Kant und Helmholtz, populärwissenschaftliche Studie. L. GOLDSCHMIDT. Hamburg, Voss. 1898. Pp. xvi+135. M. 5.

L'Éducation rationnelle de la volonté, son emploi thérapeutique. P. E. LÉVY. Preface by BERNHEIM. Paris, Alcan. 1898. Pp. v+234. Fr. 4.

NOTES.

THE American Psychological Association will meet at Columbia University, New York, beginning on Wednesday, December 28th. The American Society of Naturalists and the affiliated societies will meet at the same time and place. The 'psychology of belief' will probably be chosen as the subject for special discussion before the Psychological Association.

AT the anniversary meeting of the American Association for the Advancement of Science held at Boston in August Professor Cattell gave the address as vice-president before the Section of Anthropology, his subject being 'The Advance of Psychology' (published in *Science*, October 21). Mrs. Franklin presented a paper on 'The New Theory of the Light Sense' which will be published in this REVIEW. Before the Section of Physics the vice-president, Professor Whitman, chose a subject of interest to psychologists 'Color-vision' (published in *Science*, September 9). It may also be noted, as further showing the interest still taken by physicists in the subject of vision, that the recent presidential address by Sir George Stokes before the Victoria Institute, London, was on 'The Perception of Color.'

DR. ROBERT ZIMMERMANN, formerly professor of philosophy at Vienna, died on September 1st at Salzburg, in his seventy-fourth year. He had made important contributions to æsthetics and other departments of philosophy, being one of the leaders of the Herbartian school.

W. M. URBAN, PH.D., of Princeton, has been appointed professor of philosophy in Ursinus College, Collegeville, Pa.

DAVID R. MAJOR, PH.D. (Cornell) and B. B. Breeze, A.M. (Harvard) have been appointed fellows in the Teachers College, Columbia University. Mr. Breeze has been for the past two years assistant in the Harvard Psychological Laboratory.

PROFESSOR BALDWIN's book on 'Social Interpretations' is being translated into French by Professor Duprat, of Angély, for the Bibliothèque Sociologique Internationale (Paris, Giard et Brière), and also into German by Dr. Ruedemann (Strassburg).

ALL communications for the editors of THE PSYCHOLOGICAL REVIEW, together with books, reprints, etc., intended for review, should be sent during the year beginning November 1, 1898, to Professor J. Mark Baldwin, Princeton, N. J.



INDEX OF NAMES.

Names of contributors are printed in SMALL CAPITALS and the page numbers of the contributions in Full Face type. In the case of authors reviewed the page numbers are in *italics*, and in case of mention in the notes they are in Roman type.

- | | |
|---|---|
| Abbott, A. H., 154 | Dodge, R., 449 |
| Allen, G., 211 | Donney, J., 109 |
| ALLIN, A., 88, 109, 228 | DRURY, F. M., 55 |
| ANGELL, J. R., 179, 579 | Dumas, G., 102 |
| Anstruther-Thompson, C., 103 | Durkheim, E., 232, 424, 447 |
| Apáthy, L., 339 | Eldridge-Green, F. W., 220 |
| ARMSTRONG, JR., A. C., 519, 650 | Ellis, A. C., 341 |
| Arrer, M., 90 | Ellis, H., 230, 341 |
| ASHLEY, M. L., 595 | Evans, E. P., 229 |
| Bakewell, C. M., 449 | Fauth, F., 342 |
| BALDWIN, J. M., 1, 68, 112, 148, 164, 175, 208, 213, 218, 230, 231, 313, 341, 342, 409, 446, 671. | Féré, Ch., 530 |
| Barnes, E., 344 | Ferrari, G. C., 95, 438, 545, 547 |
| Benardini, C., 545 | Filehne, W., 539 |
| Bergson, H., 336 | FITE, W., 109, 209 |
| Binet, A., 77, 428, 659, 662, 667 | FOLSOM, C. F., 55 |
| Bigham, J., 449 | Forel, A., 109 |
| BLISS, C. B., 659 | FRANKLIN, C. L., 154, 309, 330, 416, 503, 532 |
| Bolton, F. E., 449, 548 | FRANZ, S. I., 153, 431, 548, 662 |
| Boole, M. E., 426 | Frey, M. von, 109 |
| Bosanquet, B., 227 | FRENCH, F. C., 155, 510, 655 |
| Brown, E. H., 218 | GARDINER, H. N., 100, 204, 530 |
| BUCHNER, E. F., 321 | GERMANN, G. B., 341 |
| Buck, A. F., 109 | Gomperz, H., 105 |
| Burk, F., 549 | Groos, K., 344 |
| Burmeister, E., 93 | Guicciardi, G., 95, 438 |
| Calderwood, H., 108 | Haddon, A. C., 232 |
| CALDWELL, W., 158, 401 | Haidenhain, 108 |
| CALKINS, M. W., 98, 451, 654, 674 | Hall, G. S., 87, 88 |
| CATTELL, J. MCK., 70, 110, 111, 151, 176, 411, 428, 501 | Hammond, W. A., 159 |
| Chrysostom, Bro., 155 | Helmholtz, H. von, 416 |
| Cohn, J., 97, 436 | Henri, V., 428, 442 |
| Cornelius, H., 321 | Herbart, J. F., 342 |
| CRAWFORD, J. F., 63 | Hering, E., 503, 532 |
| DAVIES, H., 183 | Herrick, C. L., 445 |
| DEARBORN, G. V., 99, 339, 445, 505, 550 | Hess, C., 333, 503, 532 |
| Deffner, K., 674 | Heymans, G., 94 |
| DEGARMO, C., 344, 668 | HIBBEN, J. G., 105, 162, 211 |
| DELABARRE, E. B., 90, 335, 673 | Hillebrand, 223 |
| Delage, Y., 213 | Hodgson, R., 420 |
| Dessoir, M., 109 | Hodgson, S. H., 232 |
| Dickson, E. T., 554 | Huey, E. B., 673 |
| | HYLAN, J. P., 439 |
| | HYSLOP, J. H., 149, 362, 522 |

- Irons, D., 100
 JAMES, W., 194, 420, 344
 JASTROW, J., 75, 172, 279, 307, 414, 518
 Johnson, M., 109
 JONES, J. W. L., 528
 JUDD, C. H., 223, 286, 388, 449, 535

 KENNEDY, F., 342, 442, 449, 477
 KIRKPATRICK, E. A., 110, 654
 Kirschmann, A., 153
 von Kries, J., 330
 Külpe, O., 209

 Ladd, G. T., 183, 518
 Lancaster, E. G., 232
 LAY, W., 438, 673
 Leclère, A., 667
 Lee, V., 103
 Leuba, J. H., 228, 449
 Lindley, E. H., 89, 90
 LLOYD, A. H., 105, 161, 326
 Lukens, H. T., 218

 MACDONALD, A., 408
 MACDOUGALL, R., 157, 167, 232, 463
 McLennan, S. F., 109
 MacTaggart, J. E., 109
 MAHOOD, E. W., 579
 De Manacéine, M., 76
 Mayhew, D. P., 99
 Meinong, A., 535
 MILLER, D. S., 197, 449
 Morgan, C. L., 213
 Mulliner, B. C., 342
 MUNROE, W. S., 68
 MÜNSTERBERG, H., 226, 411, 500, 639, 645, 668

 NEWBOLD, W. R., 336, 443
 Nichols, H., 99, 550

 PACE, E. A., 76, 99
 PALMER, G. H., 513
 Partridge, G. E., 90
 PATRICK, G. T. W., 555, 671
 Paulsen, 110
 Petersen, H. G., 443
 Pfänder, A., 639
 PIERCE, A. H., 226, 233, 539
 Pillon, F., 447

 QUANTZ, J. O., 434

 Read, C., 522
 Ribot, Th., 204
 Ritchie, D. G., 554

 Rivers, W. H. R., 232, 554
 Romanes, G. J., 213
 ROYCE, J., 113
 Russell, J. E., 109

 Sanctis, de L., 444
 Sanford, E. C., 414
 Santayana, G., 232, 344
 SCRIPTURE, E. W., 72, 152, 431, 441, 655
 SEASHORE, C. E., 547, 548
 Selby-Bigge, L. A., 105
 Seth, A., 197
 SETH, J., 227, 447, 450
 SHELDON, W. H., 426
 Sherman, F. D., 335
 Singer, E. A., 156
 SLOSSON, E. E., 652
 SMITH, W. G., 97, 220
 Sommer, R., 548
 SPRAY, J. N., 579
 STANLEY, H. M., 218, 254
 STEIN, G., 295
 Stein, L., 525
 Stern, L. W., 98
 Stern, P., 328
 Stevens, W. Le C., 554
 Stewardson, L. C., 109
 STRATTON, G. M., 536, 632
 Street, J. R., 89
 SUMNER, F. B., 616

 TAWNEY, G. A., 77
 Titchener, E. B., 400, 519, 655
 THORNDIKE, E. L., 87, 229, 449, 551, 645
 TOSTI, G., 347, 424, 525
 TRACY, F., 549
 Tschermak, A., 309
 TUFTS, J. H., 313, 409

 URBAN, W., 163, 326, 328, 677

 Vaschide, N., 659, 662

 Waldstein, L., 528
 WARREN, H. C., 70, 72, 95, 444, 543, 545, 547, 667
 Wetterstrand, O. G., 75, 443
 Wilde, H., 450
 WILDE, N., 103, 109, 344
 Wilson, L. N., 446
 WOLFE, H. K., 26
 Wreschner, A., 441
 Wundt, W., 208, 513, 536

 Zimmerman, R., 677

INDEX OF SUBJECTS.

- Abnormality, Mental, 505
 Aesthetics, 103, 328
 After-images, 333
 Alcometer, 408
 Animal Intelligence, 551
 Année psychologique, 77, 659
 Apparatus, Sorting, 279; pain, 408; movement, 548
 Association, 438, 674; and Memory, 654
 Attention, Fluctuation, 439, 444
 Automatism, Mental, 90; Motor, 295

 Belief, A Statistical Study of, 616
 Biology, Recent, 213

 Child Study, 446, 549
 Color-Blindness, 330, 503, 532
 Comparative Psychology, 229, 551
 Consciousness and the Unconscious, 650

 Depth-perception, 90, 223, 388
 Description of an Object, 667
 Drawings, Children's, 218
 Dying, Mental State of the, 530

 Education, Philosophy of, 341; Psychology and, 342, 645, 668
 Emotion, 100, 102, 543
 Ethics, 105, 227, 228, 229, 230, 447; Wundt's, 513
 Evolution of General Ideas, 204
 Eye Movements, 673

 Fatigue, 428, 547, 662, 667
 Fears, 87

 Growth of Children, 549

 Habit and Attention, 179
 Hypnotism, 75, 443

 Idealism, Dynamic, 326
 Illusions, Optical, 93, 94, 226, 233, 286, 539, 548
 Imagary, Music, 463
 Individual Psychology, 95, 172, 547
 Inhibitory Effect of Sensation, 545
 Invention, 113, 307

 Judgment of Weights, 26

 Laboratory, Studies: Harvard, 55, 295; Princeton, 63; Wellesley, 451; Chicago, 579; The Psychological, 655
 Light, Intensity of, in Visual Estimates of Depth, 595
 Logic, 105; Read's, 522

 Mathematical Psychology, 426
 Matter and Memory, 336
 Measurements, Physical and Mental, 172
 Memory, 97, 220, 436, 477; and Association, 451, 654; for Music, 545
 Methods, Psychophysical, 441
 Mind, The Story of the, 671
 Moral Education, 89
 Movement, Apparatus, 548
 Musical Ability and Disease, 545

 Nervous Condition of Students, 55
 Neural Histology, 339
 Neurological, 445
 New Books, 107, 230, 343, 448, 553, 676
 Nitrous-Oxide, Effect on Consciousness, 194
 Notes, 108, 231, 344, 449, 554, 677

 Ocular Movements, 535

 Paramnesia, Retarded, 652
 Personality, Secondary, 555
 Philosophy, 197, 447; Külpe's Introduction, 209
 Pseudoscope, A Mirror and the Limit of Visible Depth, 632
 Psychical, Research, 362, 420, 528; Fact, What is a? 645
 Psychological, Association, 145; Laboratory, 655
 Psychology, The New, 72; Wundt's Outlines, 208; as Science of Experience, 321; Danger from Experimental, 411; Experimental Course, 414; and Education, 500, 645, 668; Experimental, in Undergraduate Course, 510; Ladd's Outlines, 518; Titchener's Primer, 519; and Physiology, 550; in the Primary School, 659
 Psychomotor Problem, 99
 Purkinje Phenomenon, 309, 335
 Puzzles, 89

- Reaction-Time, 70
Reading, Psychology of, 434; and
Physiology, 673
Reflex-Time, 99
Religion, Origin of, 211; Psychology
of, 254
Secondary Personality, 555
Selective Thinking, 1
Self, Titchener's View of, 401
Sex, Psychology of, 341
Size Judgments, 547
Sleep, 76
Social Consciousness, 68; Psychol-
ogy, 313, 347, 409
Sociology, 447; Stein on, 525
Space, Perception, 442, 536; Dermal
and Optical, 579
Suicide, 424
Temperature Sense, 63
Tickling, Laughing, 88, 218
Time and Consciousness, 98
Trans-subjective, 183
Vision, 416
Will, Psychology of the, 639
Yale Studies, 431

